This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

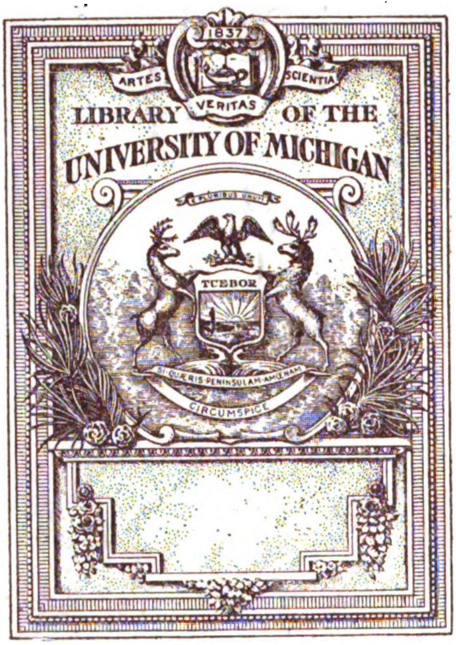
+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`


1837
ARTES
VERITAS
SCIENTIA
LIBRARY OF THE
UNIVERSITY OF MICHIGAN
E PLURIBUS UNUM
TUEBOR
SI QUÆRIS PENINSULAM AMŒNAM
CIRCUMSPICE

~~Social~~
~~SCIENCE LIBRARY~~

PSYCHOLOGICAL REVIEW PUBLICATIONS

Psychological Monographs

EDITED BY
JAMES ROWLAND ANGELL, 522 FIFTH AVE., NEW YORK
HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, NEW YORK (*J. of Exp. Psych.*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*) and
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

VOLUME XXVIII

1920

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.)
PARIS (16 rue de Condé)

BF
1
.P972
v.28

UNIV. OF MICH.

Vol. XXVIII
No. 1

PSYCHOLOGICAL REVIEW PUBLICATIONS

Whole No. 123
1920

THE
Psychological Monographs

EDITED BY
JAMES ROWLAND ANGELL, University of Chicago
HOWARD C. WARREN, Princeton University (*Review*)
JOHN B. WATSON, Johns Hopkins University (*J. of Exp. Psych.*)
SHEPHERD I. FRANZ, Govt. Hosp. for Insane (*Bulletin*) and
MADISON BENTLEY, University of Illinois (*Index*)

Quantitative Aspects of the Evolution of Concepts

An Experimental Study

BY
CLARK L. HULL, Ph.D.
Instructor of Psychology, University of Wisconsin

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

Agents: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W. C.)
Paris (16 Rue de Condé)

Psychological Review Publications

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*J. of Exp. Psych.*)
JAMES R. ANGELL, UNIVERSITY OF CHICAGO (*Monographs*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*)
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews, notices of books and articles, psychological news and notes, university notices, and announcements, appears monthly, the annual volume comprising about 480 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 480 pages. (Suspended temporarily.)

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in May, and may be subscribed for in connection with the periodicals above, or purchased separately.

ANNUAL SUBSCRIPTION RATES

Review and Bulletin: $6. (foreign, $6.50). **Journal**: $3.25 (foreign, $3.50).
Review and Journal: $6. (foreign, $6.50). **Bulletin**: $3.25 (foreign, $3.50).
Journal and Bulletin: $6. (foreign, $6.50).
Review, Bulletin and Journal: $9. (foreign, $9.75).
Index with any other two: $7. (foreign, $7.50).
Review, Bulletin, Journal and Index: $10. (foreign, $10.75).

Current Numbers: Review, 65c; Bulletin, 35c; Journal, 65c; Index, $1.25.

THE PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. THE MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages, with a uniform subscription price of $5.50. (Postal Union, $5.80.)

Philosophical Monographs: a series of treatises more philosophical in character.
Library of Genetic Science and Philosophy: a series of bound volumes.

Subscriptions, orders, and business communications may be sent direct to the

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, NEW JERSEY

FOREIGN AGENTS: G. E. Stechert & Co., London (2 Star Yard, Cary St., W. C.);
Paris (16, rue de Condé)

TABLE OF CONTENTS OF VOLUME XXVIII.

1. Quantitative Aspects of the Evolution of Concepts. An Experimental Study. Clark L. Hull. Pp. 85.

2. An Experimental Analysis of a Case of Trial and Error Learning in the Human Subject. George S. Snoddy. Pp. 78.

3. Work with Knowledge of Results versus Work without Knowledge of Results. Awareness and Partial Awareness as Factors Conditioning Efficiency. George F. Arps. Pp. 41.

4. Individual Differences in Finger Reactions. Esther L. Gatewood. Pp. 43.

5. The Lag of Visual Sensation in its Relation to Wave Lengths and Intensity of Light. Marion A. Bills. Pp. 101.

6. Studies from the Psychological Laboratory of the University of Chicago—Some Factors Determining the Degree of Retroactive Inhibition. Edward S. Robinson. Pp. 57.

7. The Higher Mental Processes in Learning. John C. Peterson. Pp. 121.

(This volume includes Monographs 123-129.)

379685

QUANTITATIVE ASPECTS OF THE EVOLUTION OF CONCEPTS[1]

INTRODUCTION

The functional and quantitative aspects of the evolution of concepts are at present in about the same state as were corresponding aspects of memory when Ebbinghaus published his monograph on that subject in 1885. This is true in spite of a very considerable activity on the part of experimenters in the field of the higher mental processes within recent years. The problem of generalizing abstraction has itself been directly attacked a number of times, notably by Moore (*8*) in 1905, by Grünbaum (*2*) in 1906-07, and by Miss Fisher (*1*) in 1912-13. But in every case the studies have been largely introspective in method, analytic in purpose, and qualitative in result. The functional and quantitative aspects of the problem remain almost untouched.

The reason for such a condition is of course not far to seek. It lies in the great complexity of the process involved. Where many factors must be cooperating nearly or quite simultaneously within a single living process, it is not easy to keep all of them constant except one and at the same time to vary that one so as to make a direct quantitative comparison of the results of the several phases of its variation. Difficulty or ease of solution of a problem is, however, often largely an expression of the efficiency of the methods and technique available. Memory which had previously resisted the efforts of psychologists, became relatively accessible after Ebbinghaus invented a suitable method. It is hoped that the quantitative aspects of the evolution of concepts will ultimately yield to a similar technique. The present study is an effort to elaborate such a technique and to apply it to a number of the characteristic quantitative problems concerned with the evolution of concepts.

---

[1] Offered in partial fulfillment of the requirements for the degree of Doctor of Philosophy, University of Wisconsin, 1918.

The work began in 1912 with a year of preliminary experimentation in the Psychological Laboratory of the University of Michigan. This was continued the following year at the Eastern Kentucky State Normal School. In 1914 the work was transferred to the Psychological Laboratory of the University of Wisconsin, where the problem and technique soon took on their final form and where the work was continued until the spring of 1918. A preliminary report containing a description of the general technique and the results from a few subjects was submitted to the University of Wisconsin as a Master's thesis in 1915. An abstract of this work was also presented in a paper before the Chicago meeting of the Psychological Association of that year, together with a demonstration of an imperfect model of the apparatus used.

II

ANALYSIS OF THE EXTERNAL CONDITIONS UNDER WHICH CONCEPTS ARE EVOLVED[1]

For the results of a psychological experiment to be taken as characteristic of any actual life-process, it is obvious that the experiment must duplicate all of the conditions essential to that process. In comparatively simple processes especially where much is already known concerning them, the difficulty of determining in advance what are the essential characteristics, is not excessive. But in a highly complex and imperfectly known process such as that by which concepts are evolved, it may fairly be said to be great. Theoretically, perhaps the only way to decide positively whether a given factor is influential or not is to determine experimentally whether any different result is obtained in cases where *all* possible factors are included as compared with cases where all except this one are included. And for similar reasons, in the absence of such experimental determination with all other factors equal, the more completely the conditions of the life-process are duplicated in the experiment, the greater the probability that the results of the experiment will be true of the particular life-process. It is consequently incumbent on us at the very outset of such an investigation to make as complete and careful an analysis as possible of the external conditions which precipitate and condition the process in question.

No doubt concepts may be evolved in many different ways (*10*) just as associative learning may be performed in many different ways. But the evolution of concepts or generalizing abstraction, being a distinctly more complicated process, may reasonably be expected to present a considerably greater variety of forms. Some of these are comparatively simple, while others are extraordinarily complex. One of the simplest and most primitive types of generalizing abstraction may be illustrated by the following experiment.

---

[1] The customary historical survey is omitted here chiefly because of the paucity of material bearing upon our present problem. Contact is made with various studies in the course of the monograph, in the most appropriate connections.

The writer held a lighted white paraffine candle in an otherwise dark room, about a foot from the face of a lively child of sixteen months. The child soon put her hand forward into the flame and extinguished it. When the candle was lighted again a moment later, she shrank away from it with a characteristic turning-away of the head, a peculiar facial expression and a drawing back of the hands against the sides of the body. The next day she was held up to an 8 C.P. glowing electric light bulb hanging from a cord. She had been fond of playing with this under similar circumstances for some weeks. But now she refused to touch it, responding with a similar turning-away of the head, the peculiar facial expression and the drawing back of the hands against the sides of the body as in the candle experiment. A moment later when the current was turned off and the bulb was no longer luminous but still warm, she took it in her hands as readily as ever. This was repeated several times on the following day with uniform results. Record was made on the same day.

Assuming that the candle experience was the cause of the changed reaction to the electric light bulb, it is evident that a reaction has been transferred from the former to the latter, presumably through the identical element of luminosity. The mere *abstraction* of this element from the gross complex of the candle situation resulted simply from the superior attention value of the brightness. The *generalization* resulted from a major associative bond being formed between the luminosity and the reaction of withdrawing. The changed reaction to the luminous lamp merely made outwardly manifest the *generalizing abstraction* which had already taken place.

A second characteristic type of generalizing abstraction is involved in the so-called inductive "steps" of formal instruction originated by Herbart. These briefly summarized are:

1. The subject is presented with a number of experiences, either simultaneously or in succession, each of which contains a certain characteristic common to the others.

2. The subject is brought to compare deliberately the various situations presented with a view to discovering similarities and differences among them.

3. Lastly the significant element common to all the experiences is deliberately sought out, found, and formulated in language.

Here the process is much more elaborate than the type previously described: a linguistic reaction replaces the protective; more than one concrete situation is used as a basis of the process; the act of generalization becomes for the subject a deliberate task, and largely an end in itself. The process has strong elements of artificiality, as will be pointed out in connection with the third and "standard" type of generalizing abstraction to be described presently. This of course implies nothing as to the usefulness of the "steps" as a method of instruction.

The third will be called the "standard" method of generalizing abstraction because it is believed to be the method by which the great majority of actual concepts are evolved. It is consequently the method which is made the basis of the experimental investigations to be described later and for that reason will be analyzed in much fuller detail than the others. In schematic outline the external factors conditioning this process may be briefly stated as follows: A young child finds himself in a certain situation, reacts to it by approach say, and hears it called "dog." After an indeterminate intervening period he finds himself in a somewhat different situation, and hears that called "dog." Later he finds himself in a somewhat different situation still, and hears that called "dog" also. Thus the process continues. The "dog" experiences appear at irregular intervals. The appearances are thus unanticipated. They appear with no obvious label as to their essential nature. This precipitates at each new appearance a more or less acute *problem* as to the proper reaction. This problem largely monopolizes the focus of consciousness. Meantime the intervals between the "dog" experiences are filled with all sorts of other absorbing experiences which are contributing to the formation of other concepts. At length the time arrives when the child has a "meaning" for the word dog. Upon examination this meaning is found to be actually a characteristic more or less common to all dogs and not common to cats, dolls and "teddy-bears." But to the child the process of arriving at this meaning or concept has been largely unconscious. He has never said to himself, "Lo! I shall proceed to discover the characteristics common to all dogs but not enjoyed by cats and 'teddy-bears'." The formation of the concept has never been an end deliberately sought for itself (*7*, 204). It has always been the means to an end—the supremely absorbing task of physical and social reaction and adjustment. Such in brief is our "standard" or normal type of concept evolution.

It will be convenient at this point to consider the methods followed by the three experimenters who have attacked the problem of generalizing abstraction. According to our preceding analysis, all of them fall considerably short of fulfilling the conditions for the evolution of concepts, as it usually takes place in ordinary life.[2] This is particularly true of the studies of Moore and Grünbaum.

Moore[3] presented to his subjects in rapid succession, by means of a Ranchberg memory apparatus, groups of nonsense characters. One-fourth second was given to each exposure with a blank period of equal length between. Each group contained five distinct characters arranged in a horizontal row. One character was the same in all the groups, all the other characters different. The subjects were directed to determine as quickly as possible whether there was any common element in the series, and to terminate the series of exposures by pressing a key the instant this was realized. The subjects were then directed to report the recollection of their mental states during the process, particularly as to the nature of the common element found and the other characters seen.

Grünbaum (*2*) modified the method of Moore somewhat. He presented to his subjects simultaneously within a single visual field, two groups of simple distinct characters. One character was alike in each of the two groups, all the others different. The two groups of characters were separated by a diagonal line. The exposures were for three seconds. The subjects were directed to first find the character common to the two groups as quickly as possible and then to examine the other characters during the remainder of the exposure. The subject then reported the nature of the character found, his recollection of the process of finding, and any other characters which he could remember.

---

[2] Their findings so far as they apply to the present study, will be considered as occasion arises.

[3] Op. cit.

In the first place, at least for an adult, the groups used by both the above experimenters must have contained an extremely weak unity even where the exposures were as brief as those used by Moore. The rôle of language in the process is utterly ignored, and the problem and reaction aspects are lacking in the sense in which they appear in the normal process. Moreover the generalizing is made a deliberate task instead of being incidental. Lastly there is the extreme brevity of the total process. Some of Moore's experiments average from four to six seconds, and Grünbaum's less than three seconds per generalization!

Miss Fisher (*1*) presented to her subjects visually in succession, complicated unitary nonsense forms containing common elements. The exposures were three seconds each, and there were ten forms in each series. Beneath each form of the series was printed conspicuously the same nonsense name. The subjects were told that at the end of the series they would be called upon to define the group name. Elaborate introspections were taken frequently throughout the process. Here we have a somewhat closer approximation to a *bona fide* process of concept evolution. Its approximation is to the distinctly artificial type exemplified by the Herbartian formal "steps" however, rather than to the process as it takes place spontaneously in ordinary life. Her experiences are ideally unitary. She partly recognizes the rôle of language, though its *guiding* function in generalization (*11*, 11; *14*, 149 ff. especially 161) is not realized any more than in the Herbartian "steps." Just as there, the mind of the subject is still tingling with the impression of the last experience when a new one of the same kind appears. The general nature of each new experience is known before it is presented. There consequently never exists any problem for the subject to discover a suitable reaction to the situation as such. As a further consequence, he does not utilize the results of his partial generalizations in coping with the succeeding situations. The problem so far as any exists is only a pseudo problem—that of deliberately and self-consciously discovering common elements. It is difficult to say what influence her constant and elaborate introspections had upon the process, though it is safe to assume that such an amount of irrelevant mental activity was not without its effect.

III

THE EFFICIENCY OF EVOLVING CONCEPTS BY PROCEEDING FROM THE SIMPLE TO THE COMPLEX

Experiment A

Problem

The first problem which we shall consider arises from the familiar educational injunction to "proceed from the simple to the complex." We shall try to determine experimentally how far this method justifies its reputation when applied to the evolution of concepts. How much if any more economical is it to evolve a concept by experiencing first the simpler concrete examples from which it is to be abstracted and proceeding progressively to the more complex and difficult examples, over an opposite order. In symbols, if the concept x is to be evolved from the series of concrete examples:

ax, bcx, defx, ghijx, klmnox, pqrstux,

is it more or less efficient to experience the examples in the simple-to-complex order (reading from left to right) or to experience them in the reverse or complex-to-simple order?

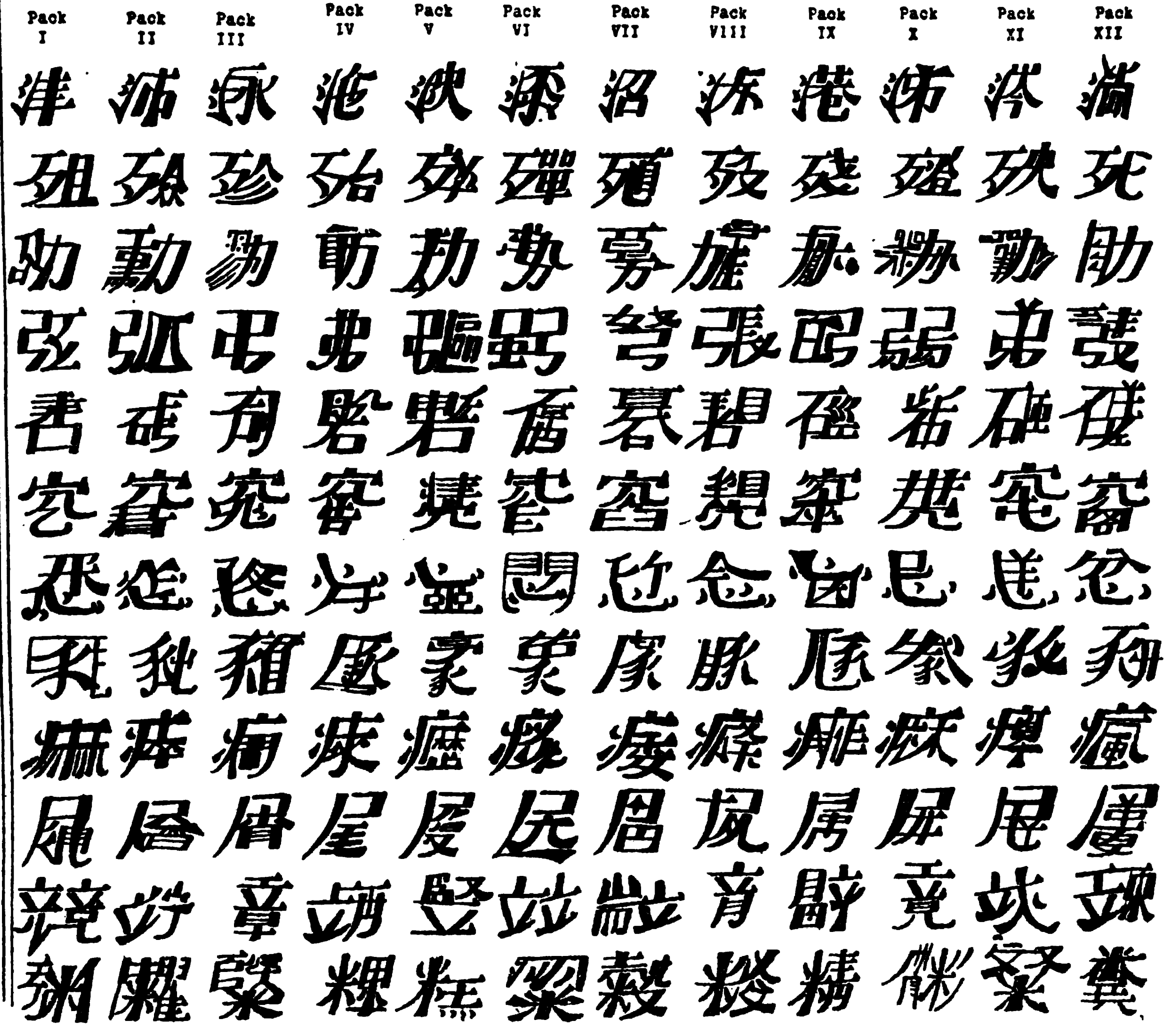
Technique

The general conditions under which concepts are ordinarily evolved have been roughly suggested in the last chapter. We must now formulate them more exactly and translate them into terms of experimental technique.

1. *The subject must be presented with a large number of unitary experiences each distinct from the rest.*

The basis for such a series of visual experiences was supplied by the 144 Chinese characters shown in Plate I. These characters were taken from a Chinese dictionary, and freely adapted to the present purpose wherever deemed necessary. They were drawn with great care in black ink upon cards two inches long and one inch wide. The characters thus prepared were presented

	Word	Concept
Series A	oo	氵
Series B	yer	歹
Series C	li	力
Series D	ta	弓
Series E	deg	石
Series F	ling	穴
Series G	hui	乚
Series H	chun	豕
Series I	vo	疒
Series J	na	尸
Series K	nez	立
ries L	fid	米


Pack I
Pack II
Pack III
Pack IV
Pack V
Pack VI
Pack VII
Pack VIII
Pack IX
Pack X
Pack XI
Pack XII

to the subject one after another by means of a specially constructed apparatus.

This device somewhat resembled the drum form of Wirth memory apparatus. The slips on the drum held twelve cards. The periodic movements of the drum were controlled automatically and exactly by a simple pendulum clockwork which was built into the apparatus. By means of a special form of cam this clockwork released the drum at periods varying anywhere from one to six seconds as desired. Throughout the present study a uniform exposure-time of five seconds was employed. The movements of the pendulum were concealed from the subject by a screen of cardboard attached to the table upon which the apparatus rested. The sound of the ticking of the clockwork was reduced to that of an ordinary clock by an elastic escapement of special shape. In addition this escapement was attached to the pendulum by means of a piece of rubber which absorbed the greater part of the remaining vibration. When wound the apparatus would continue giving exposures accurately, of the required length and without any attention whatever from the experimenter for a period of from 40 to 50 minutes. The accurate automatic feature of the device was of the greatest value throughout the experiment. Indeed owing to the necessarily complex duties of the experimenter while the experiment was in progress, the study could not have been carried on without it.

2. *Each one of these unitary experiences must require a definite reaction from the subject at each presentation.*

This requirement was satisfied by having the subject respond to each character as it appeared by speaking a particular nonsense syllable.

3. *The reaction required must be identical for each individual within limited groups of experiences. The resulting groups must be numerous.*

This requirement was satisfied by having each of the twelve characters of the first horizontal row in Plate I (Series A) reacted to by speaking the word "ōō," the next row of twelve by speaking "yer" and so on. Thus each horizontal row of char-

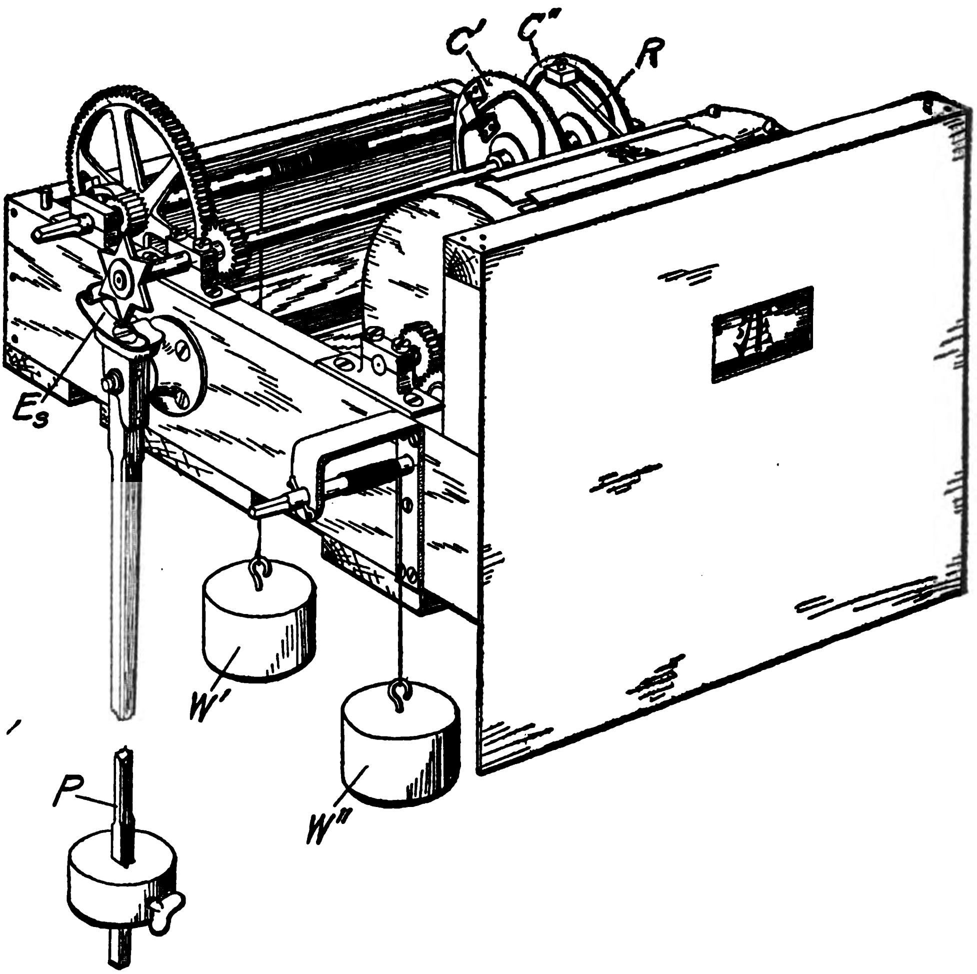


PLATE II. EXPOSURE APPARATUS.—P, pendulum with adjustable bob. Es, elastic escapement. C", cam attached to the escapement shaft. This cam controls by the rod R a secondary escapement (not shown by this view) which engages pins set in the farther end of the drum. This escapement releases at equal intervals of time, permitting the drum instantly to turn one twelfth of a revolution. C' is a second cam which may be used to secure movements of the drum at intervals of time one half as great. W' and W" are lead weights which drive the clockwork and the drum respectively. The drum has at equal intervals on its surface, twelve pairs of shallow grooves in which cards bearing characters are placed for successive exposure at the window. One of the characters is shown at the window as viewed by the subjects in experiments A to I.

acters constituted a separate reaction-group. There were twelve such groups in all, which are regarded as sufficiently "numerous" to satisfy the technical requirements of the experiment.

The twelve syllables used were pronounced as follows: ōō, yêr, lē, tă, dĕg, lĭng, huĭ, chŭn; vō, nă, nĕz, fĭd.

4. *All of the individual experiences which require a given reaction, must contain certain characteristics which are at the same time common to all members of the group requiring this reaction and which are NOT found in any members of the groups requiring different reactions.*[1]

This requirement is satisfied by the fact that all the characters (e.g. in Series A Plate I), contain certain strokes in common. These characteristic strokes for each of the twelve reaction-groups are isolated in the vertical column of characters labeled "concept." They may be easily observed imbedded in each of the characters in the group following but they are not found in any of the other groups.

5. *All of the individual experiences which require a given reaction must always be accompanied by the experience of a given word, which must never accompany experiences requiring a different reaction.*

This was accomplished in the early part of each experiment by the experimenter pronouncing the name in question in the middle of the exposure of each of the particular characters of a given reaction-group whenever they chanced to appear. The ticking of the clockwork marked the middle of the five-second exposure. The subject always repeated the syllable distinctly immediately after the experimenter. He was directed to anticipate the experimenter as soon as possible by himself speaking the required word during the two-and-one-half seconds of the exposure before the prompting should occur. A consistent reaction of this kind was always established after a longer or shorter series of exposures. In cases of successful reaction the subject heard the word spoken by himself in connection with the corresponding character. Consequently there was no need for the experimenter to speak the word.

---

[1] This of course ignores the obvious fact that the same object may be conceived in numerous ways and that under different circumstances may require very different reactions. Only a slight modification of the technique here employed would be necessary to measure its influence which presumably is important. (See James, *4*, II, 333.)

6. *The individual concrete experiences must take place in a random order and consequently one unanticipated by the subject.*

This was secured as follows: In the first place the characters were presented in "packs" of 12. An inspection of Plate I will show that a "pack" or *vertical* column of characters includes one representative of each reaction-group. Since the capacity of the drum was also 12, a "pack" of characters exactly filled the drum. Secondly the order of the representatives of the various reaction-groups in a given pack was fixed by chance, subject only to the qualification that the representatives of two or more reaction-groups should never come in immediate succession in any two successive "packs," and further that the representative of a given reaction-group should never appear first or last in more than one "pack."

7. *The experiences belonging to a given reaction-group must ordinarily be separated by one or more experiences belonging to the other groups.*

This is necessary effectually to efface any immediate memory images which might otherwise be superposed directly upon a second experience of the same reaction-group. This was secured by the procedure just described under (6) above.

8. *Each new experience, particularly at the beginning of the process, must precipitate the more or less acute problem of determining what reaction will secure satisfaction and avoid annoyance.*

This was adequately realized by the unanticipated order of the experiences described above, together with the fact that the subject was under obligation if possible of reacting promptly and correctly by pronouncing the name of each character as it appeared. If he could not react correctly he had the annoyance of failure, and if he succeeded he had the satisfaction of conscious success.

9. *In coping with new situations, the subject must constantly use any incipient stages of the concept that may be in existence at a given time, however imperfect and false such embryonic concept may be.*

This was automatically realized to the maximum by the continuous necessity of reacting correctly by naming the new characters as they appeared.

10. *The concept (in the mind of the subject) must never be an end in itself but rather a means to successful reactions to concrete experiences.*

This was secured by occupying the subject's mind as completely as possible with the problem of perfecting his reaction. Except where otherwise stated, great care was taken to avoid any other motive. In the instructions, all suggestions of abstraction of common elements, generalization or definition were studiously avoided. Positively he was led to believe that he was about to do a kind of memory experiment. He was told that he would be prompted in the middle of each exposure, that he should repeat the syllable each time when prompted and that as soon as possible he was to react before being prompted. This process was continued with the first "pack" until all the reactions were perfected, which to all intents and purposes really was a memorizing experiment.[2] At the beginning of the second "pack" the subject was told in a matter-of-fact way that the second set would be easier because the same names would be used as with the first set, so there would be no new names to learn. Just before exposing the first character, he was told to see how many of the new characters he could "guess" correctly before being prompted. This injunction to "guess" was given at the beginning of each succeeding series.

11. *A definite functional unit of measure must be provided.*

A thoroughly convenient and adequate functional measure of the perfection of a concept lies in the extent of the ability to react successfully to new but analogous situations (*12, 13*). If in a given time spent one method of evolving concepts gives ability to react successfully to 50 per cent of all new situations and another gives ability to react to 60 per cent of the same or similar new situations, the second method is obviously superior to the first to a definite intelligible amount and regardless of ability to define or any other subjective consideration. Accordingly only the first six "packs" of characters (Plate I) were used for the purpose of evolving the concepts. The remaining six "packs" (VII to XII) were reserved exclusively for testing or measuring the concepts thus evolved. In making up the test "packs" the characters were rearranged from the order as they appear in Plate I in such a way that in a given "pack" or drum-full, two or even three characters of a given concept might be present and of course not any of certain others. This was to prevent subjects from determining the identity of characters by a process of elimination. Three exposures were given of each one of these test "packs" (of course without promptings) and the subjects were encouraged to "guess" at them very freely if any suggestion whatever of a possible identity came to mind. This latter was done so as to get as far as possible the liminal as well as the supraliminal reactions and thus obtain a more adequate measure. The three reactions to each of the six test-characters of a given concept gave eighteen measures for each concept and a total of 216 measures for all twelve concepts with a given subject. This was our main measure.

---

[2] Introspections taken at the conclusion of the experiments indicated that the greater part of the time of the subjects was always consumed in the immediate task of perfecting the required reaction. The most of them spent a little time however in seeking the common element. A few seem never to have attempted it.

In addition there were three supplementary measures of some importance. The first and most important of these is based upon exactly the same principle as the main measure. At the first presentation of each succeeding "pack" of the evolution series (first six "packs," Plate I) there arises the necessity of reacting to the respective characters during the two-and-one-half seconds before the prompting takes place. This obviously gives the required functional measure and at a point of peculiar interest as it takes place periodically throughout the entire evolution process and enables us to trace the progress of the process from the first. As pointed out above the subjects were encouraged to react or "guess" very freely especially during the first time through each "pack" even in cases where they were extremely doubtful.

The second supplementary measure came from the evolution series also. It was furnished by the number of promptings which were required to perfect the reactions to each of the first six "packs." Clearly the farther advanced the evolution of the concepts in general, the fewer promptings will be required.

A third supplementary measure of the stage of the evolution of concepts, though by no means a functional one, is obtained by requiring the subject to define the concepts (by drawing them) at the end of the process. If desired these definitions may be called for at any time during the process though that might easily introduce a disturbing factor. For this reason it was never done except in special experiments carried out for this particular purpose.

Certain defects appear in all four of the measures described above. In the chief measure and in the first two supplementary measures the element of chance introduces a slight error. Subjects were encouraged to "guess" at doubtful cases and chance alone would give on the long run one correct guess out of twelve. This is but a slight possibility and its influence is reduced to insignificance by the fact that on the average only about 10 per cent to 15 per cent of the reactions were chance in this sense. But whatever influence actually resulted from this was effectually eliminated from the results by the fact that it was equal for each of the two sets of concepts compared.

In the first and second supplementary methods a slight error is introduced by the fact that the subject was sometimes able to react successfully to a character through a process of elimination. This error is slight however and in the long run may be expected to be equal for each of the two sets of concepts, and so can hardly disturb the results. These two supplementary measures are of distinctly less reliability than the main measure because they are based upon a so much smaller number of measures. And of the two the second, not being strictly functional in nature, is of distinctly less value than the first. The third supplementary measure, that of definition, is rather qualitative in its nature and is not functional. It has no unit of measure in any ordinary sense though it is possible to extract from such drawings a certain kind of quantitative value (see p. 43). Nevertheless this method may furnish important information concerning the evolution of concepts, and it might profitably be more fully employed than is the case in the present study.

12. *The disturbing influences of past training and experiences on the part of the subjects must be eliminated.*

This was effectually secured by the use of the strictly unfamiliar nonsense-material described above.

13. *The effects of individual differences as to the rate of learning among various subjects, of practice, of fatigue, of ennui, of obscure physical or psychic indispositions and spurts, and all sorts of other factors of a similar nature, which often seriously disturb quantitative experiments, must be completely equalized.*

This was accomplished at a single stroke by having the concepts of the two methods to be compared, evolved by the *same* subject and *simultaneously*. The advantage of this procedure can scarcely be overestimated. Six of the total twelve concepts were accordingly evolved by the simple-to-complex method and six by the complex-to-simple method. Thus if a subject is naturally slow, his native slowness appears equally in the simple-to-complex method and in the complex-to-simple method; and if he is slightly fatigued or fresh, his fatigue or freshness is absolutely constant for both methods. The writer regards this as the most important single feature of the technique employed.

14. *The technique must be so flexible that it may be possible to vary in a definite way any factor involved in the process without disturbing the constancy of any of the other factors.*

The requirement of varying the direction of the change of complexity in the various evolution series without disturbing any of the other factors, presented difficulties. These will be considered more in detail as the various means of avoidance are described from time to time. In the main the requirement was satisfied very simply. As previously pointed out, only the first six "packs" were used for the purpose of evolving the concepts, and are therefore spoken of as the "evolution" series just as the other six "packs" are spoken of as the "test" series. Now a glance at the evolution series of concept A (Plate I) will show that in the first character the common element stands out very clearly, it appears less clearly in the second, still less clearly in the third and fourth characters and least clearly of all in the fifth and particularly in the sixth character. In short the characters are arranged in the order of increasing complexity, difficulty or general obscurity of the common element. This we have called "simple-to-complex." The same is true of series, B, C, D, E, and F, which together make up half of the total twelve concepts. On the other hand a similar examination of the first six characters making up the evolution series of concepts G to L, the other half of the total twelve, will show that they are at first most complex and gradually become simpler and simpler from "pack" to "pack" as one moves from left to right, being most simple or clear at the sixth. In short these last six evolution series are arranged in the order of *decreasing* complexity or general obscurity of the common element. This we have designated as "complex-to-simple" order. From the above it may be seen that in experiencing the evolution "packs" in the order I, II, III, IV, V, VI, series A to F will be evolved in the simple-to-complex order while series G to L will be evolved simultaneously in the complex-to-simple order, and the same amount of time will be spent upon each of the two groups of concepts.

The method of determining the position of the respective characters in the series of increasing and decreasing complexity was based upon the general impression of clearness or obscurity of the common element as judged by the experimenter after having drawn them.[3] The twelve characters of a given concept series were carefully arranged in a row according to increasing complexity or obscurity. From this row the first, third, fifth, eighth, tenth and twelfth characters were taken and made respectively the first, second, third, fourth, fifth and sixth character of the evolution series. The rest were thrown by chance into the test series.

15. *The results must be protected from any distortion which might arise from lack of equality in the difficulty of the material employed.*

---

[3] This is true of Experiments A, B and C. In Experiment D, the final one on this particular problem, the characters were arranged according to experimentally determined functional difficulty.

Differences in difficulty among the various concept-series were not avoided and no particular effort was made to do so. The aggregate or average difficulty of the two groups of concepts compared however was practically equal, as may be seen by consulting Tables I, II, and III. This was a natural result of the fairly homogeneous nature of the material. But all possible disturbance of the results by slight difference in the difficulty of the two sets of material compared, was effectually eliminated by having only half of the subjects experience the "packs" in the order of I to VI as described above, while the other half experienced them in the reverse order. In the first case concepts A to F were simple-to-complex, while G to L were complex-to-simple. In the second case concepts A to F became complex-to-simple while concepts G to L became simple-to-complex, thus completely reversing the process in both cases. Consequently if in the first group of subjects concepts A to F should give on the tests a score of errors for the simple-to-complex method slightly in excess of what strict equality in the difficulty of the groups should show, when the process is reversed and concepts A to F have become complex-to-simple, they may be expected to give the same slight excess of errors to that method also. And when the results of all subjects are combined, these two equal excesses will neutralize each other and the final result will be effectually protected from disturbance from this source.

16. *Sufficient concepts must be evolved and sufficient measures taken from each to (largely) eliminate chance variation.*

This was approximately achieved by using with each subject six concepts and 108 measures on the final tests for each of the two methods compared, besides the secondary measures which furnish reliable corroborative evidence. In the most of the experiments later described ten subjects were used, giving a total of 2,160 measures on the tests above. In the four experiments on the economy of the simple-to-complex method of evolving concepts alone over 8,600 measures were taken.

An exact record was made of every reaction of the subject as the experiment was performed. Special mimeographed blanks were provided for the purpose. A plus sign was recorded for every correct reaction, a minus sign for every failure to react and the nature of the response was recorded in every incorrect reaction. These later are of course counted exactly as minus signs in the final tables. An exceedingly careful system of key letters and numbers was used to prevent errors in prompting, scoring, etc.

Except where otherwise stated all the experiments throughout the study were conducted in a small research room in the psychological laboratory of the University of Wisconsin. The room had one large window of northern exposure. The apparatus sat on the top of a sort of table 42 inches high and 16 inches wide, where the light of the window fell directly upon the exposed characters. The latter were about on a level with the subjects' eyes. The experimenter sat on a high stool behind the table and at one side of the apparatus. Short rest-periods were permitted between the various "packs" to avoid fatigue. The experiments described in the present chapter ordinarily consumed about three hours each. This included the rest-periods.

The subjects in the immediate experiments and in all the experiments except where otherwise stated were university students. In most cases they had little or no training in psychology and were thoroughly naïve. Ordinarily they regarded the work as a kind of memory experiment and were mainly interested in making a good score. It is safe to say that not one of the subjects used in this form of the experiment had the slightest suspicion that the ultimate purpose of the experiment was to compare the efficiency of two distinct modes of concept evolution. When informed of this at the end of the experiment they never failed to express more or less astonishment. For the above reasons no form of suggestion arising from preconceived theoretical considerations could enter to distort the results. Indeed the writer regards such subjects as distinctly more valuable than trained psychologists for such an experiment as the present one.

We may now briefly summarize the salient features of the technique described above with special reference to its application in Experiment A: The twelve characters of "pack" I are inserted in the twelve holders of the drum. The drum goes round and round without interruption making successive exposures of each character of exactly five seconds. The characters are stationary while being viewed. The experimenter pronounces the concept-name of each character at the middle of its exposure and the subject repeats it with care. After the first revolution of the drum the subject himself begins to react to the characters by pronouncing the name wherever possible, before being prompted. At length every character will be reacted to correctly at a single revolution of the drum. The characters are at once removed from the drum and replaced but in a *different* order. The prompting is then resumed as before until the subject has made a perfect score on each of two successive revolutions of the drum.

Then "packs" II, III, IV, V, and VI which comprise the evolution "packs" are inserted and learned one after the other. In these cases however, the "packs" are only learned in *one* order each, and to a perfect score on two successive revolutions of the drum. Less and less time is required for each succeeding "pack."

In order to equalize any possible inequality of difficulty inherent in concepts A to F as compared with the concepts G to L, half of the subjects learned the reactions to the "packs" in the order of VI, V, IV, III, II, I. In this case "pack" VI was learned in two orders by these subjects just as "pack" I was by the first half of the subjects.

Lastly the six test "packs" are presented. They are inserted on the drum one after another and merely given three revolutions each, i.e., three exposures to each character. The subject reacts wherever possible, no promptings being given. Sometimes the subjects were then asked to draw on a specially prepared blank the characteristics which a character must contain in order to be called "ōō," "yêr," "lē," etc.

Results

The results of the tests show a distinct advantage for the simple-to-complex method. They are given in a condensed form by subjects in Table I. There it appears that seven of the ten subjects show a more or less decided advantage for the simple-to-complex method. Out of a possible 108 errors in reacting to the tests of the simple-to-complex series, an average of 30.2

TABLE I

Showing the relative efficiency of the simple-to-complex method, Experiment A.

Subject	Order of experiencing evolution "packs"	Number of failures to react correctly on test series, out of possible 108. Simple-to-complex	Complex-to-simple	Advantage of simple-to-complex
Eat.	I to VI	9	34	+25
S. B.	" " "	27	35	+ 8
Mac.	" " "	62	60	— 2
M. B.	" " "	8	49	+41
Mau.	" " "	34	59	+25
Lie.	VI to I	14	36	+22
Pau.	" " "	25	19	— 6
Wan.	" " "	23	46	+23
Sch.	" " "	48	33	—15
Dav.	" " "	52	70	+18
Average		30.2	44.1	+13.9
M. V.				14.1
S. D.				16.3

failures was made, as against an average of 44.1 failures on the tests of the complex-to-simple series. This gives an average advantage of 13.9 points for the simple-to-complex method.[4]

The results are confirmed by both of the first two supplementary methods of measurement described on p. 15. The results of the first supplementary measure are shown in Table II. There

TABLE II

Showing the number of failures to react correctly at the first presentation of the characters of evolution "packs" by the respective methods.

		Total number of failures to react correctly at first presentation.											
		Simple-to-complex ("packs" given in chronological order)						Complex-to-simple ("packs" given in chronological order)					
Subject	Order of experiencing "packs"	I	II	III	IV	V	VI	I	II	III	IV	V	VI
Eat.	I to VI	6.0	3.7	3.0	2.0	.2	2.0	6.0	5.0	5.0	3.5	2.8	1.2
S. B.	" " "	6.0	2.0	1.0	1.0	.5	2.5	6.0	5.5	4.5	3.5	4.5	3.5
Mac.	" " "	6.0	3.0	4.0	4.0	5.0	4.0	6.0	3.0	5.0	4.0	4.0	2.0
M. B.	" " "	6.0	5.5	2.5	1.5	1.0	2.0	6.0	5.0	3.8	3.5	2.5	3.0
Mau.	" " "	6.0	3.0	3.0	2.0	3.0	2.0	6.0	5.0	4.0	3.0	3.0	5.0
Lie.	VI to I	6.0	2.0	3.0	2.0	2.0	1.5	6.0	4.0	3.0	4.5	3.0	2.0
Pau.	" " "	6.0	5.0	5.0	5.0	5.0	2.0	6.0	6.0	5.0	1.0	3.0	4.0
Wan.	" " "	6.0	4.5	3.0	3.5	2.0	1.0	6.0	5.5	5.0	2.0	1.5	.5
Sch.	" " "	6.0	3.0	4.0	4.0	4.0	2.0	6.0	6.0	6.0	5.0	2.0	3.0
Dav.	" " "	6.0	5.0	3.5	4.0	4.5	3.5	6.0	5.0	3.0	4.0	2.0	3.5
Average		6.0	3.7	3.2	2.9	2.7	2.25	6.0	5.0	4.4	3.4	2.8	2.77

---

[4] A computation of the "unreliability" of this average shows that there is one chance in about 286 that there is no advantage for this form of the simple-to-complex method. (Thorndike, E. L., Mental and Social Measurements, Chap. XIII.)

appear in chronological order the number of failures to react correctly at the first presentation of the respective evolution "packs," for the simple-to-complex and the complex-to-simple methods respectively. Owing to the small number of measures taken on a given "pack" with a single subject, there is considerable variation in the figures in the body of the table. The final averages however show a decided and consistent advantage for the simple-to-complex method. This appears very clearly in Figure I where the relation of the two sets of averages is shown graphically. These curves show not only a distinct superiority for the simple-to-complex method but at what an early stage in

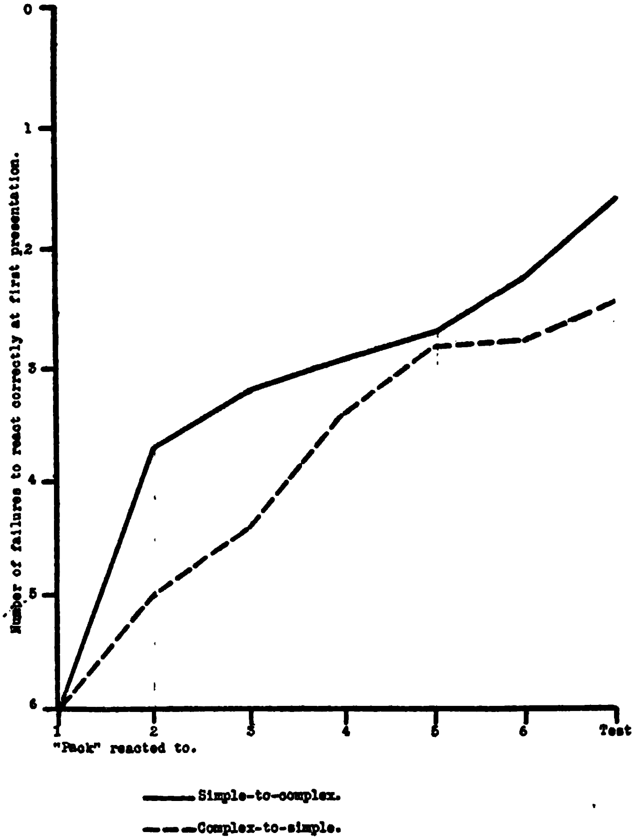


FIGURE I.—Showing the rate of improvement in ability to react to new situations, evolution series, Experiment A.

the process this superiority is established. Already at the beginning of the second "pack," it is greater than at any other time in the process. Indeed there is a distinct tendency for the curves to converge near "pack" VI. This however is doubtless due largely to the fact that the characters of the upper (simple-to-complex) curve are at their maximum difficulty at this point, thus tending to increase faulty reactions and so to make the curve fall. On the other hand the characters of the lower curve are at their minimum of difficulty which tends to make a minimum of faulty reactions and so to elevate artificially the curve at this point.

The superiority of the simple-to-complex method is shown once more by the second supplementary measure. The comparative results by this method are shown in Table III. Here appear the total number of promptings required at each stage of its evolution for the six concepts being evolved simultaneously by the respective methods. As in Table II there is considerable variation in the individual scores but the final averages still show a distinct superiority for the simple-to-complex method. This is shown graphically in Figure II. Thus by all of the three methods employed, the simple-to-complex method of evolving concepts has proven the better of the two.

TABLE III

Showing the number of promptings required to perfect the reaction at each succeeding "pack" of the evolution series for the two methods compared, Experiment A.

Subject	Order of experiencing "packs"	Total number of promptings required to perfect reaction at each succeeding "pack". Simple-to-complex ("packs" given in chronological order)						Complex-to-simple ("packs" given in chronological order)					
		I	II	III	IV	V	VI	I	II	III	IV	V	VI
Eat.	I to VI	32	12	5	1	0	1	39	26	12	4	3	1
S. B.	“ “ “	47	6	2	0	2	4	37	19	13	5	6	4
Mac.	“ “ “	39	13	9	13	8	7	43	13	9	13	9	3
M. B.	“ “ “	51	8	5	6	2	2	33	12	10	20	6	4
Mau.	“ “ “	26	3	4	6	3	4	32	10	7	6	4	7
Lie.	VI to I	24	2	5	3	3	1	26	5	5	5	1	3
Pau.	“ “ “	24	18	13	10	8	3	23	20	15	4	3	5
Wan.	“ “ “	23	10	11	7	4	0	28	26	17	4	1	0
Sch.	“ “ “	60	12	16	11	8	6	43	15	15	9	4	5
Dav.	“ “ “	37	14	13	12	10	7	44	24	8	8	2	7
Average		36.3	9.8	8.3	6.9	4.8	3.5	34.8	17.0	11.1	7.8	3.9	3.9

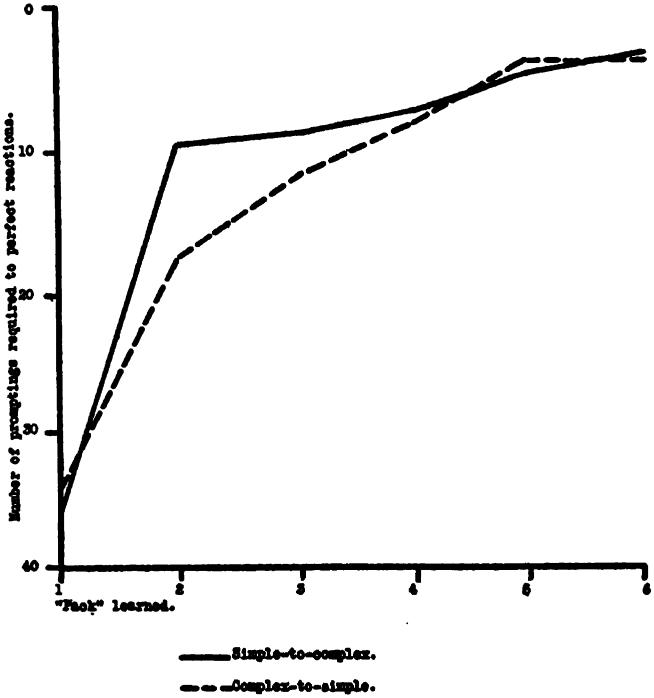


FIGURE II.—Showing the number of promptings required to perfect the reaction at each succeeding "pack," evolution series, Experiment A.

On page 18 it is pointed out that it was difficult to keep all other factors equal while varying the single one of the direction of change in complexity of the material. In the present experiment the time-element presented difficulties from this source. It is true that according to the technique employed the total time consumed by each of the two methods was bound to be absolutely equal. Despite this, an important time-relation remained grossly unequal. An examination of the distribution of time among the various "packs" (p. 30) shows that about four times as much was given to the simple end of the simple-to-complex evolution series as to the simple end of the other. Clearly this alone might account for the advantage obtained for the simple-to-complex method in the present experiment if it be assumed that the simple end of such a series is more efficient in the evolution of functional concepts per unit time. That such is actually the case is strongly suggested by the fact that the entire advantage gained by the simple-to-complex method is secured on the first one or two "packs" (Figure I), where an excessive amount of time is spent upon the easy end of those series.

Accordingly three new experiments were instituted. One (Experiment B) reduced the disproportion of time spent upon the simple ends of the respective series evolved by the two methods. In the other two (Experiments C and D) it was equalized entirely.

EXPERIMENT B

In this experiment the same material, apparatus and general technique were employed as in Experiment A, except that instead of learning the first (chronological) "pack" in two orders, it was here learned in only *one*. All "packs" were learned as usual until a perfect score was made on two successive revolutions of the drum. Thus the only difference between the present experiment and the one just described lies in reducing the time spent upon the first "pack." Eight subjects were used in the experiment.

The results in general confirm those found in Experiment A though upon a somewhat reduced scale. The smaller advantage here shown for the simple-to-complex method tends strongly to confirm our suspicions as to its origin which were expressed above. The results are shown by subjects in Table IV. Six of

TABLE IV

Showing the relative efficiency of the simple-to-complex method, Experiment B.

		Number of failures to react correctly on test series, out of possible 108.		
Subject	Order of experiencing evolution "packs"	Simple-to-complex	Complex-to-simple	Advantage of simple-to-complex
Rig.	I to VI	36	38	+ 2
Bot.	" " "	49	43	— 6
Wey.	" " "	31	18	—13
Hen.	" " "	40	54	+14
Cop.	VI to I	29	44	+15
Dum.	" " "	6	20	+14
Sym.	" " "	42	47	+ 5
M. C.	" " "	33	71	+38
Average		33.2	41.9	+ 8.7
M. V.				11.6
S. D.				14.56

the eight subjects show a more or less striking tendency to greater efficiency by the simple-to-complex method. The final averages show a score of 33.2 failures to react out of a possible 108 as compared with 41.9 failures by the complex-to-simple method. This gives a net advantage of 8.7 points for the simple-to-complex method[5] against one of 13.9 by the method of Experiment A — a perceptible reduction.

These results are further confirmed by the two secondary measures which show the relative efficiency of the two processes from the beginning. The increase in ability to react to new cases throughout the evolution series is of special significance and is shown for the two methods separately in Table V. The final averages of the two series are shown graphically in Figure III. While showing the superiority of the simple-to-complex method, it also furnishes additional confirmation of our suspicion concerning its cause. The curves of Figure III, just as in Figure I, show that the rise in the upper or simple-to-complex curve takes place almost entirely during the first part of the process, ("packs" I, II and III) which, it will be remembered, is its easy end. On

TABLE V

Showing the number of failures to react correctly at the first presentation of the successive evolution "packs" for the respective methods, Experiment B.

The number of failures to react correctly at first presentation.

Subject	Order of experiencing "packs"	Simple-to-complex ("packs" given in chronological order)						Complex-to-simple ("packs" given in chronological order)					
		I	II	III	IV	V	VI	I	II	III	IV	V	VI
Rig.	I to VI	6.0	3.5	2.5	2.0	2.0	4.0	6.0	5.5	6.0	3.0	3.5	3.5
Bot.	" " "	6.0	5.0	3.0	3.5	3.5	4.0	6.0	4.5	5.0	2.5	4.5	3.0
Wey.	" " "	6.0	2.0	3.0	3.0	2.0	2.0	6.0	6.0	5.0	4.0	1.0	3.0
Hen.	" " "	6.0	1.0	1.0	.0	.0	.0	6.0	6.0	6.0	5.0	5.0	2.0
Cop.	VI to I	6.0	4.0	3.0	3.5	2.0	2.0	6.0	6.0	5.0	4.0	4.0	4.0
Dum.	" " "	6.0	2.5	2.0	2.0	2.0	.0	6.0	5.0	3.5	2.5	2.0	1.0
Sym.	" " "	6.0	5.0	3.0	3.0	3.0	4.0	6.0	6.0	3.0	4.0	2.0	3.0
M. C.	" " "	6.0	5.0	3.0	4.0	3.0	4.0	6.0	4.0	5.0	5.0	4.0	5.0
Average		6.0	3.5	2.5	2.6	2.2	2.5	6.0	5.4	4.8	3.8	3.2	3.1

---

[5] A computation of the "unreliability" of this average shows that there is one chance in about 22 that the "true" average would not show an advantage for the simple-to-complex method.

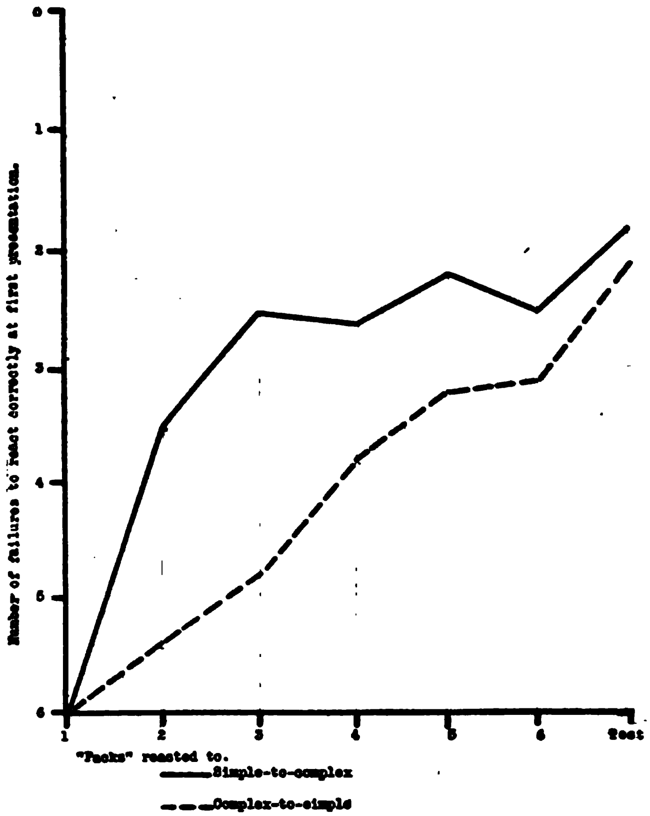


FIGURE III.—Showing the rate of improvement in ability to react to new situations, evolution series, Experiment B.

the other hand the lower or complex-to-simple curve, makes a much greater comparative rise at "packs" IV, V and VI which is also *its* easy end. Much of this is clearly due merely to the difference in difficulty of the characters presented for reaction, but not all. At "pack" IV, in spite of the fact that the simple-to-complex characters are here slightly the more difficult of the two, the curve shows almost twice as great an advantage for the method as was shown by the tests, where the material is equally difficult for both methods. Evidently from the fourth "pack" on, the simple characters of the complex-to-simple series have proven more efficient in evolving functional concepts than the complex characters of the simple-to-complex series, for they have greatly reduced the advantage of the simple-to-complex method which was present at the end of the third "pack." Given more time on the last three "packs," this advantage might be completely eliminated. Clearly then, any method which spends a greater proportionate time upon the simple or more efficient end of the series, will tend strongly for that reason alone to be a more efficient method, and vice versa. As already pointed out the smaller advantage obtained for the simple-to-complex method in the present experiment accords with this view. The average number of minutes spent upon each "pack" in the two experiments is as follows:

Pack	I	II	III	IV	V	VI
Experiment A	17.9	9.2	7.6	5.8	5.1	4.8
Experiment B	17.0	10.8	10.4	7.6	7.3	6.5

Thus in Experiment A the two "packs" at the simple end of the complex-to-simple series received only 33 per cent as much time as the corresponding end of the simple-to-complex series, while in Experiment B this inequality is reduced so that the ratio is 50 per cent.

TABLE VI

Showing the number of promptings required to perfect the reaction at each succeeding "pack" of the evolution series for the two methods compared, Experiment B.

		Number of promptings required at each succeeding "Pack."											
		Simple-to-complex ("packs" given in chronological order)						Complex-to-simple ("packs" given in chronological order)					
Subject	Order of experiencing "packs"	I	II	III	IV	V	VI	I	II	III	IV	V	VI
Rig.	I to VI	97	31	13	7	1	15	78	39	29	12	19	10
Bot.	" " "	40	24	13	14	3	12	36	35	16	24	14	9
Wey.	" " "	40	4	6	4	3	4	40	14	16	8	2	3
Hen.	" " "	61	13	32	6	13	3	88	25	35	16	27	15
Cop.	VI to I	46	27	15	8	5	3	55	36	21	9	3	16
Dum.	" " "	13	4	3	2	1	0	15	10	7	5	3	1
Sym.	" " "	28	29	11	7	8	7	30	19	12	9	3	5
M. C.	" " "	35	18	9	5	4	5	60	16	10	10	8	12
Average		45	19-	12.8	6.6	4.7	6.1	50.2	24.2	18.2	11.6	9.9	8.9

The promptings throughout the evolution series are shown in Table VI. The final averages by the two methods show a consistent advantage for the simple-to-complex method. They appear graphically in Figure IV.

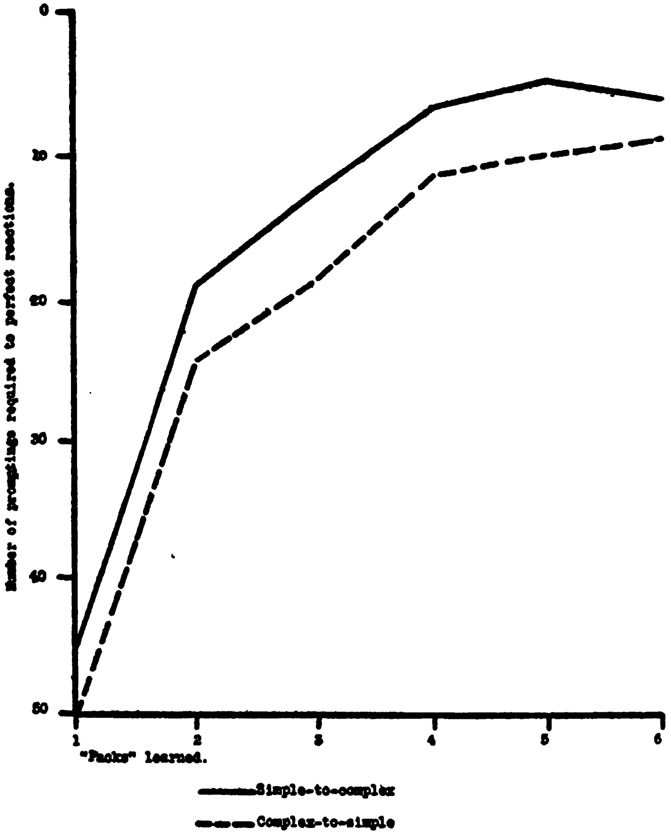


FIGURE IV.—Showing the number of promptings required to perfect the reaction at each succeeding "pack," evolution series, Experiment B.

EXPERIMENT C

In setting up this experiment the main object was to equalize the time-elements as completely as possible for the simple halves of the two groups of series compared. Obviously the only way to do this was to give the same number of exposures to every pack of the evolution series. After considerable deliberation, nine exposures per "pack" were selected as the number most likely to yield results comparable with the two previous experiments. It was the average number of exposures actually given in Experiment B.

In thus equalizing the time element, some fears were entertained lest a second and equally disturbing factor might be introduced which would have a somewhat similar effect, viz. an unequal distribution of attention and effort. It is of course evident from the number of exposures necessary to be given to the different "packs" in Experiment B (p. 30) that the average subject will not have perfected his learning of the first two "packs" at the ninth exposure but will have completed the learning of the *last* two "packs" some time *before* the ninth exposure. Other things equal it might reasonably be expected that there would be a certain amount of dimunition of the attention in the latter packs after the reaction had been perfected. In order to prevent this if possible, rest periods were introduced between each of the "packs." And when the subject had perfected the learning before the ninth exposure he was warned to maintain the same degree of attention as before. In this connection it must be pointed out also that association bonds are by no means at their maximum strength when they are just capable of producing the reaction in two and one-half seconds. Considerable effort on the part of the subject is still required to make the correct response and the continued repetitions show their influence in the increased speed of the reactions.

After all possible precautions have been taken there still remains doubt upon this point. But clearly any disturbing effect of this factor will be favorable to the simple-to-complex series. If the results still show a tendency to greater economy by the simple-to-complex method, the meaning will be ambiguous, as it might be due to either of the two possible variable factors involved. But if the advantage should disappear it would be strong confirmation of our previous results pointing to the greater amount of time spent upon the more efficient half of the simple-to-complex series as the cause of the greater efficiency which was found for that method.

The results of the tests show this to be the case. They appear in Table VII. While there is, as usual, considerable variation among the individual subjects, about as many favor one method as the other. The final average for all subjects gives an almost identical score for both methods. The difference is only one

TABLE VII

Showing the relative efficiency of the simple-to-complex method, Experiment C.

		Number of failures to react correctly on test series, out of a possible 108.		
Subject	Order of experiencing evolution "packs"	Simple-to-complex	Complex-to-simple	Advantage of simple-to-complex
Tay.	I to VI	28.5	27.0	— 1.5
Hag.	" " "	70.5	44.5	—26.0
Bon.	" " "	46.0	32.0	—14.0
Fin.	" " "	11.0	5.0	— 6.0
Rei.	" " "	79.0	86.0	+ 7.0
Nor.	VI to I	21.0	18.0	— 3.0
Old.	" " "	35.0	58.0	+23.0
Tor.	" " "	37.0	57.0	+20.0
Eva.	" " "	49.0	32.0	—17.0
Osm.	" " "	79.0	85.0	+ 6.0
Average		45.6	44.45	— 1.1
M. V.				12.1
S. D.				14.87

point, and even this is reversed and in favor of the complex-to-simple method.

As in the previous experiments, the results of the supplementary measures support those of the tests. The comparative functional efficiency of the two methods in reacting to new cases is shown from the beginning of the process in Table VIII. The final averages of the two methods are shown graphically in Fig-

TABLE VIII

Showing the number of failures to react correctly at the first presentation of the successive evolution "packs" for the respective methods, Experiment C.

		The number of failures to react correctly at first presentation											
		Simple-to-complex ("packs" given in chronological order)						Complex-to-simple ("packs" given in chronological order)					
Subject	Order of experiencing "packs"	I	II	III	IV	V	VI	I	II	III	IV	V	VI
Tay.	I to VI	6.0	4.0	2.0	3.0	2.0	4.0	6.0	4.5	3.5	2.0	1.0	.5
Hag.	" " "	6.0	6.0	4.5	4.0	2.0	4.5	6.0	6.0	3.0	4.0	5.0	4.0
Bor.	" " "	6.0	5.0	3.0	4.0	4.0	3.5	6.0	6.0	3.5	4.5	2.5	1.0
Fin.	" " "	6.0	6.0	5.0	1.0	1.0	2.5	6.0	5.0	3.5	3.0	2.5	1.0
Rei.	" " "	6.0	5.0	4.0	5.0	4.5	3.0	6.0	5.5	4.5	6.0	5.0	2.5
Nor.	VI to I	6.0	6.0	3.5	2.0	2.0	.5	6.0	6.0	5.0	3.0	2.0	2.0
Old.	" " "	6.0	4.5	2.5	4.0	5.0	3.0	6.0	5.0	4.0	4.0	3.0	5.5
Tor.	" " "	6.0	5.5	3.5	4.0	2.5	3.0	6.0	5.0	4.5	3.5	2.5	4.0
Eva.	" " "	6.0	5.0	4.5	3.0	2.0	2.5	6.0	4.5	4.5	3.0	2.0	3.0
Osm.	" " "	6.0	5.5	6.0	3.5	6.0	5.5	6.0	5.5	5.5	4.5	5.0	5.5
Average		6.0	5.25	3.85	3.35	3.1	3.2	6.0	5.3	4.15	3.75	3.05	2.9

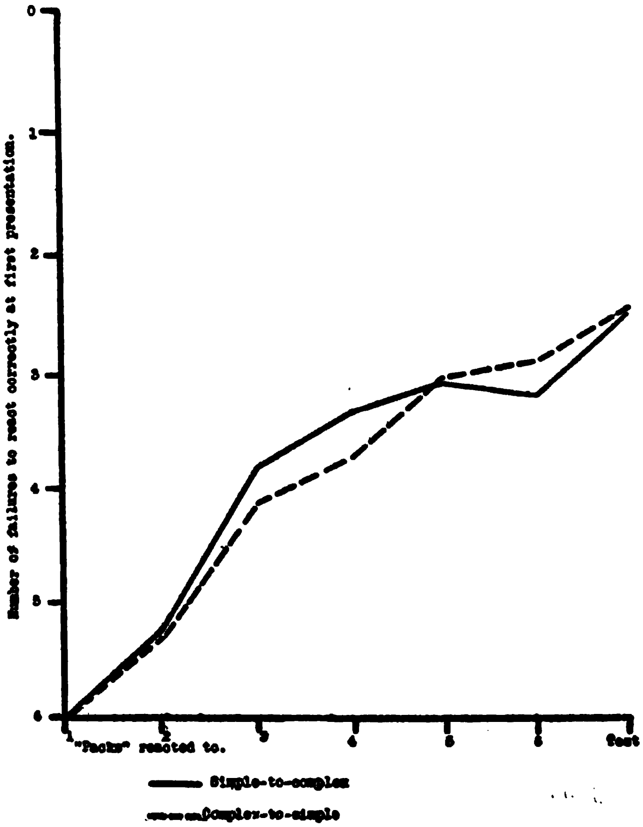


FIGURE V.—Showing the rate of improvement, in ability to react to new situations, evolution series, Experiment C.

ure V. The two curves follow an almost identical course. But the differences, small as they are become significant when taken in connection with the corresponding processes of Experiments A and B. (Figures I and III.) Here we find again that the simple-to-complex method rises more rapidly at first. At "pack" IV it attains its greatest advantage in spite of the fact that its characters are the more difficult of the two at this point. But the remainder of the curve shows that this advantage is entirely lost during the second half of the evolutionary process as was anticipated above. No advantage appears by the simple-to-complex method as such.

EXPERIMENT D

Owing to the importance of the question under investigation, it was thought desirable to repeat Experiment C under more rigid conditions. It will be recalled that the characters of the evolution series in the previous experiments were arranged according to the complexity or general obscurity of the common elements as determined by the subjective impression of the experimenter. In the present experiment the criterion of arrangement was the actual functional difficulty of the individual characters. This was readily determined for the characters of the original *test series* by computing the aggregate number of failures to react correctly for each character as shown by the test records of 25 subjects. The characters of the original test series were accordingly converted into a new evolution series while the old evolution series became a new test series. This criterion has the distinct additional advantage that it shows relatively *how great* the increase or decrease of complexity or difficulty is, which we are dealing with. The average increase in percent of failures for the entire group is approximately 5 per cent from step to step and fairly uniform:

20.5%, 25.6%, 29.7%, 34.8%, 41.8%, 54.7%

TABLE IX

Showing the relative efficiency of the simple-to-complex method, Experiment D.

		Number of failures to react correctly on test series, out of possible 108.		
Subject	Order of experiencing evolution "packs"	Simple-to-complex	Complex-to-simple	Advantage of simple-to-complex
Joh.	I to VI	65	44	—21
And.	" " "	10	22	+12
Woo.	" " "	65	57	— 8
Mur.	" " "	61	72	+11
E. H.	" " "	46	68	+22
Sch.	" " "	23	17	— 6
Hem.	VI to I	55	37	—18
Adl.	" " "	39	63	+24
Cra.	" " "	11	33	+22
Pet.	" " "	79	71	— 8
M. H.	" " "	70	66	— 4
Tig.	" " "	45	44	— 1
Average		47.4	49.5	+ 2.08
M. V.				13.3
S. D.				15.0

And while an inspection of the characters according to the new arrangement makes it possible that we have here a factor slightly different from that aimed at in the previous experiments, it is also probable that we are dealing with a distinctly more fundamental and significant type of complexity.

The results of the experiment confirm in every detail those already obtained. When the same amount of time is spent on each member of the evolution series, there is no advantage in favor of proceeding from the simple to the complex. Of the twelve subjects, seven show an advantage for the complex-to-simple method while an average of all gives a negligible advantage of 2.09 in favor of the simple-to-complex method with a mean variation of 13.4. The score is shown by subjects in Table IX. The course of the successes by the two methods as shown by the initial reaction to the characters of the evolution series is given in Table X. The averages of the two methods are plotted in Figure VI. The characteristic features of the corresponding curves of Figure V are here repeated in detail though on a greatly increased scale.[6]

TABLE X

Showing the number of failures to react correctly at the first presentation of the successive evolution "packs" for the respective methods,

Experiment D.

Subject	Order of experiencing "packs"	The number of failures to react correctly at first presentation.											
		Simple-to-complex ("packs" given in chronological order)						Complex-to-simple ("packs" given in chronological order)					
		I	II	III	IV	V	VI	I	II	III	IV	V	VI
Joh.	I to VI	6	5	5	5	4	5	6	5	6	4	4	3
And.	" " "	6	0	0	0	1	3	6	6	2	2	1	1
Woo.	" " "	6	6	6	5	3	4	6	4	4	5	3	2
Mur.	" " "	6	6	6	4	4	4	6	6	5	4	3	2
E. H.	" " "	6	6	5	5	3	6	6	6	6	5	2	3
Sch.	" " "	6	2	0	2	1	1	6	4	4	2	0	0
Hem.	VI to I	6	4	3	4	3	6	6	6	6	3	4	3
Adl.	" " "	6	6	5	4	4	3	6	5	5	6	4	3
Cra.	" " "	6	4	4	3	2	3	6	4	5	4	4	3
Pet.	" " "	6	4	5	4	5	6	6	5	5	5	4	4
M. H.	" " "	6	5	4	6	5	4	6	6	6	5	3	4
Tig.	" " "	6	5	4	3	3	3	6	6	5	3	3	2
Average		6.0	4.4	3.9	3.7	3.2	4.0	6.0	5.3	4.9	4.0	2.9	2.5

---

[6] This probably reflects the greater psychological potency of the present method of arranging the characters.

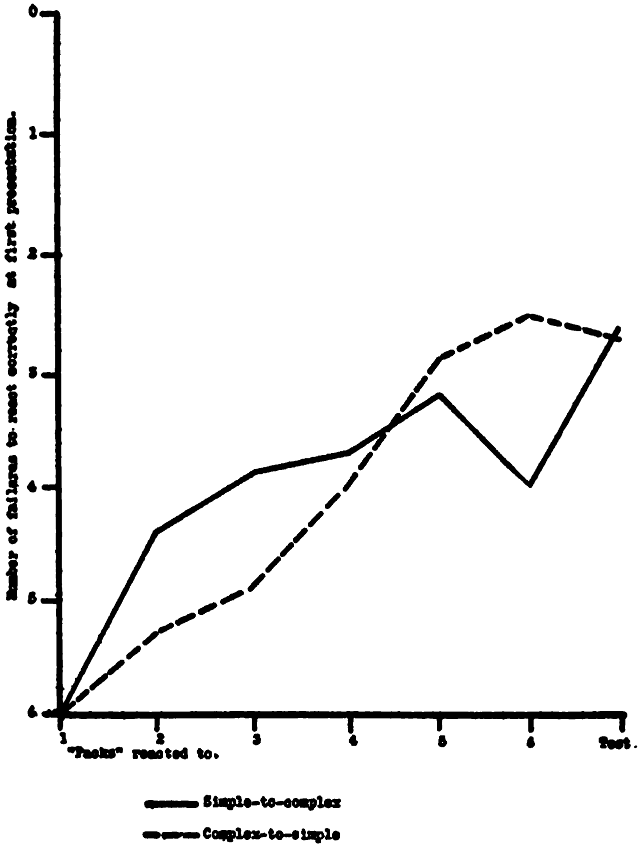


Figure VI.—Showing the rate of improvement in ability to react to new situations, evolution series, Experiment D.

As there, the simple-to-complex method shows a distinctly better score on "packs" II, III and IV, the curves cross between "packs" IV and V, and the complex-to-simple method shows a better score on "packs" V and VI, only to come together on the test, which was the only place in the series where the material reacted to was of the same difficulty for both methods. As pointed out in previous experiments the advantage shown for the respective methods in the various "packs" was largely due to the actual difference of difficulty in the characters reacted to in each case. But if this factor alone were operating the curves should have crossed between "packs" III and IV instead of later. At "pack" IV we know that the simple-to-complex characters are about 5 per cent more difficult than the complex-to-simple. This acting alone would have reversed almost exactly the positions of the two curves. The fact that they were not reversed shows clearly that the simple characters of the simple-to-complex method have been distinctly more efficient than the complex characters of the complex-to-simple method up to that point i.e. during the first three "packs." That this advantage was absent at the end of the evolutionary process as evidenced by the tests, shows with equal clearness that the simple characters at the end of the complex-to-simple series worked with equal efficiency when their chance arrived, and that the order of arrangement as such probably has no influence upon the efficiency of the process whatever.

SUMMARY AND CONCLUSIONS

The results of Experiments A, B, C and D may be briefly formulated as follows:

1. In the evolution of functional concepts simple experiences are more efficient than complex ones. This appears to be true regardless of whether the simple experiences take place at the beginning or end of the process of evolution.

2. Concrete experiences in the simple-to-complex order appear to be no more efficient in the evolution of functional concepts than when in the complex-to-simple order if inequalities of the time-factor are excluded.

3. But if each individual experience in the evolution series is continued until the reaction to it is just perfected before passing to the next, there is a distinct advantage in favor of the simple-to-complex method. This is due to the fact that under this method a greater amount of time is spent upon the simple or efficient end of the simple-to-complex series than upon the corresponding end of the complex-to-simple series. It seems not improbable that whatever belief may exist as to the greater economy of the simple-to-complex order may have arisen from the natural tendency to spend a disproportionate amount of time

upon the first cases in the series just as was done in Experiments A and B. Thus understood there is a real economy by this procedure.

Caution must be exercised in applying results such as the above to the conditions of instruction, for example, and that for a number of reasons. In the first place, as pointed out above (p. 5), there are very considerable differences between the conditions of the present experiment and those usually found in formal instruction. Under such different conditions, especially in respect to the immediately successive experiencing of the cases of a given concept-series and the general lack of the functional reaction throughout the series, the relations found above to obtain between the two might be considerably altered.

A general criticism and evaluation of the method employed in the above experiments may be made at this point. The great weakness as well as strength of the present method lies in its abstractness, particularly in the use of nonsense-material. Obviously, results from such material must be verified and checked up by further experiments with the more complex material of every day life. Even there an economy might conceivably hold for one kind of material and not for another, such is the specific nature of psychological functions. An example in point is seen in Ebbinghaus' "law of forgetting." It was found to hold though upon a reduced scale, for many kinds of "intellectual" material but probably not at all for thoroughly learned muscular habits (*15*, II, 309 ff).

On the other hand the advantages of the method are very great. This is particularly true of a study which is attempting to make a preliminary exploration in a little known field such as the present. The advantage consists largely in extreme simplicity, uniformity, flexibility and general controlability of the material employed. Here again the classical work of Ebbinghaus on memory may be profitably considered. Perhaps it is not too much to say that owing largely to the abstract nature of his method and particularly to the extreme managability of his material, he made as great a contribution to our knowledge of the quantitative aspects of memory in two or three years as has resulted from all the enormous amount of work done on memory since that time. The work done since was of course just as necessary as his. The point is that by the use of abstract experiments there is an enormous economy of time and energy in reaching preliminary conclusions as to where the truth may probably be found in the complex material of everyday life. In short it sets the problem for the concrete experiment and has limited practical value until verified by the latter. But here again the history of memory experimentation may be encouraging, for hardly one of the principles worked out by Ebbinghaus has failed to find its counterpart in the complex material of every day life, though often with limitations. This suggests that even with the limitations mentioned above, a certain amount of presumption arises that the tendencies found in the present experiments may prove true even of so artificial a process as formal instruction.

IV

THE EFFICIENCY OF RECEIVING CONCEPTS WITHOUT THE LABOR OF GENERALIZING ABSTRACTION

EXPERIMENT E

A second theory of essentially quantitative implications has long been held concerning the evolution of functional concepts. This theory is that to have functional value, concepts must be evolved from the concrete by each individual for himself (6, 229). Practice has on the other hand very often (though apologetically) striven to give the concept to the individual outright. But while theory asserts stoutly the greater functional efficiency of concepts evolved by the individual himself as compared with those given to him outright, it has not been determined how much greater is the efficiency, much less how it would vary (if at all) with different amounts of time available for the process. Here then we find our problem: Is it more or less efficient to spend a given amount of time in perfecting the reaction to an abstract characteristic never seen in its concrete setting, or to spend the same amount of time perfecting reactions to a series of concrete situations with the incidental amount of generalizing abstraction?

The main features of the method utilized are the same as those employed in the experiments just described, in particular Experiment B. It differed from Experiment B in having half of the characters of the evolution series replaced by their respective common elements. That is to say, the characters comprising the six members of each of the evolution series G to L say, would be replaced in every case by the common element shown in the first vertical column headed "concept" (Plate I). The other half of the evolution series (A to F) remain exactly as before in the simple-to-complex order. Thus in learning the evolution "packs" in the order I to VI, there would be a simultaneous comparison of (1) evolving concepts by the simple-to-complex method

TABLE XI

Showing the relative efficiency on the tests of the two methods of evolution being compared, Experiment E.

Subject	Concept given outright in:	Number of failures to react correctly on the tests out of a possible 108. Concept given outright	Simple-to-complex	Advantage of giving concept outright
Smi.	Series A to F	58	71	+13
Dec.	" " " "	39	61	+22
San.	" " " "	55	47	− 8
Cod.	" " " "	26	53	+27
Kiv.	" " " "	45	26	−19
Oma.	Series G to L	54	55	+ 1
Hjo.	" " " "	65	71	+ 6
Col.	" " " "	70	46	−24
Clo.	" " " "	87	87	0
Sta.	" " " "	37	45	+ 8
Average		53.6	56.2	+ 2.6
M. V.				12.8
S. D.				15.13

(Series A to F) with (2) receiving concepts in the abstract apart from their concrete setting (Series G to L). As in the previous experiments the necessity of equalizing any differences in difficulty which might be inherent in the two groups of concepts compared made it necessary to reverse the process with half of the subjects. In this case the naked common elements were given in Series A to F and the simple-to-complex method in Series G to L. Otherwise the experiment was exactly as in Experiment B, each "pack" being learned to a perfect score on two successive revolutions of the drum.

Ten subjects were used in the experiment. They were university students from a class in introductory psychology.

Contrary to the expectation of the experimenter, there was no advantage for either of the two methods compared. The results of the tests are shown in Table XI. As usual the variation among the individual subjects is considerable but about as many favor one method as the other. The final averages are almost equal, there being a difference of only 2.6 points. This however is in favor of the method of giving the concepts outright. But in view of a S.D. nearly five times as great, this has slight significance.

From the nature of the evolution series in this experiment no supplementary measure was obtained from this source. However, at the conclusion of the respective experiments, five of the subjects (two of the first group and three of the second) were asked to draw on a specially prepared blank their idea of what a character must contain to be called by the various names such as "ōō," "yer," "le," etc. Even to casual inspection, these drawings show a very striking superiority for the concepts which were given outright. In order to get a quantitative determination of the extent of this superiority, four persons of considerable psychological training compared each drawing with the corresponding one in the first column of figures of Plate I and then scored the drawings on a scale of ten. The scoring was done without knowledge of the method used to produce the individual drawings. The average score of the respective groups of concepts as thus determined is shown by subjects in Table XII. The final averages show that where the concepts are given outright, the defining ability is much greater in proportion to functional ability than where they are abstracted by the subject. Where the concepts are evolved by the subject himself, the percentage of efficiency in definition is eight points *less* than the functional efficiency by the same process, while by the abstractly given concepts the per cent of efficiency in definition is eighteen points *more* than the functional efficiency attained by the same method. The score on the "definitions" by the "given" method, is nearly thirty points higher than its functional efficiency would require by the method of actual evolution by the subject.

TABLE XII

Showing the relative ability to define by the two methods of acquiring concepts, Experiment E.

		Relative functional efficiency		Relative efficiency in definition	
Subject	Concept given outright in:	Concept given outright	Simple-to-complex	Concept given outright	Simple-to-complex
Dio.	Series A to F	64%	43%	55%	38%
Cod.	" " " "	76%	51%	89%	77%
Oma.	Series G to L	50%	49%	68%	32%
Hjo.	" " " "	40%	34%	73%	18%
Col.	" " " "	35%	57%	71%	27%
Average	....	53.0%	46.8%	71.2%	38.4%

The results of the experiment may be briefly summarized as follows: Where the evolutionary process has reached an average efficiency of approximately 50 per cent,

1. Concepts given outright show a functional efficiency equal to concepts evolved by the subject himself from concrete cases in the simple-to-complex order.

2. The ability to define the concept by drawing is about twice as great where the concepts ~~are given outright~~ as where they are evolved from the concrete by the subjects themselves.

3. From (1) and (2) follows the corrollary that ability to define is not necessarily a true index of the functional value of a concept (*17*, 141 ff).

It must be noted that the above experiment covers only one of three or four possible cases of the present problem. It may be that very early in the process, say at a point where only about 20 or 25 per cent of efficiency is attained, the advantage would be distinctly with the "given" concepts that at an efficiency of around 50 per cent (the present case) there would be no consistent advantage for either method; and that at a normal efficiency of about 90 per cent the advantage will pass over decidedly to the simple-to-complex method.[1]

There is a further limitation upon the generality of the above results even for the region of a 50 per cent development of the process. Above we have likened the giving of the concepts in the present experiment to the process of formal instruction by definition, which educational reformers have inveighed against so much. In reality the present method probably suffers from only one of the two evils with which instruction by definition is afflicted. One is its *abstractness* and the other is that the *verbal* nature of definitions brings it about that even the abstract meaning of the definition is *imperfectly comprehended.* In the present experiment only the *abstractness* was involved, for presumably the abstract element itself was immediately and completely experienced.

---

[1] Since this was written, additional experiments along the lines here indicated, show that some at least if not all of the above conjectures will be realized. The writer hopes to complete these experiments and publish the results as a separate study in the near future.

V

THE EFFICIENCY OF EVOLVING CONCEPTS BY A COMBINED METHOD OF ABSTRACT PRESENTATION AND CONCRETE EXAMPLES

Experiment F

In the last chapter we found reason to suspect that mere abstract information is of rather doubtful efficiency in coping with new situations in the later stages of the process of generalizing abstraction. Where the process has been approximately half completed, its efficiency is about the same as by the simple-to-complex method. The problem accordingly arises: May not a combination of the two methods be more efficient than either alone? Specifically, is it more or less efficient to evolve functional concepts by first receiving the abstract characteristic and then a concrete case, or to have the subject evolve it entirely for himself by the simple-to-complex method?

The method of attacking the problem was very similar to that employed in Experiment E. Two changes were made to adapt it to the present problem: In the first place all the characters of evolution series G to L were reversed so that they stood normally in the simple-to-complex order in exactly the same sense as series A to F (Plate I). After being reversed the characters falling in "packs" I, III and V were replaced by the naked common elements used in Experiment E. Otherwise the learning was exactly as usual, to a perfect score on two successive revolutions of the drum. In this way series A to F would be experienced in the ordinary simple-to-complex order. But series G to L would be experienced as: first the "concept" given abstractly, second an example, third the "concept" given abstractly again, fourth another example, fifth the "concept" given abstractly once more and lastly another example. Thus series G to L was evolved from an alternation of abstract presentation and example, the examples being in the simple-to-complex order. As usual in

TABLE XIII

Showing the functional efficiency of combining abstract presentation with the giving of concrete experiences, Experiment F.

Subject	Combination method in:	Number of failures to react correctly in the test out of a possible 108. Combination method	Simple-to-complex	Advantage of combination method
Wat.	Series A to F	38	45	+7
Fee.	" " " "	27	57	+30
Bec.	" " " "	19	28	+9
Gor.	" " " "	33	64	+31
Cro.	" " " "	1	14	+13
Out.	Series G to L	15	38	+23
Jac.	" " " "	15	28	+13
Sla.	" " " "	48	42	—6
Sur.	" " " "	28	61	+33
Whi.	" " " "	21	41	+20
Average		24.5	41.8	+17.3
M. V.				10.1
S. D.				11.8

order to avoid error from possible difference in difficulty between the two groups of concepts, the process was reversed with half of the subjects in which case series A to F were alternate abstract presentation and example, while series G to L were simple-to-complex.

The tests revealed a decided advantage for the combination method over the simple-to-complex method. The results of the tests are shown in Table XIII. All but one subject gave a distinct

TABLE XIV

Showing the number of failures to react correctly at the first presentation of the successive evolution "packs," Experiment F.

The number of failures to react correctly at first presentation.

Subject	Combination method in Series:	Combination method I	II	III	IV	V	VI	Simple-to-complex method I	II	III	IV	V	VI
Wat.	A to F	(6)	5	(3)	2	(1)	5	6	6	4	4	4	5
Fee.	" " "	(6)	6	(0)	3	(1)	6	6	6	2	3	4	4
Bec.	" " "	(6)	5	(3)	3	(0)	3	6	4	3	4	4	1
Gor.	" " "	(6)	1	(1)	1	(0)	4	6	6	6	1	3	5
Cro.	" " "	(6)	3	(0)	0	(0)	4	6	3	2	2	1	2
Out.	G to L	(6)	5	(1)	3	(0)	5	6	4	2	3	0	2
Jac.	" " "	(6)	4	(0)	3	(1)	2	6	3	3	2	1	2
Sla.	" " "	(6)	4	(3)	3	(2)	4	6	5	6	4	2	3
Sur.	" " "	(6)	5	(2)	2	(0)	4	6	6	5	4	1	4
Whi.	" " "	(6)	6	(2)	5	(1)	2	6	6	4	3	4	1
Average		(6.0)	4.4	(1.5)	2.5	(.6)	3.9	6.0	4.9	3.7	3.0	2.4	2.9

advantage for the combination method. The final averages show that the combination method gave only 24.5 failures to react successfully out of a possible 108, while the simple-to-complex method gave a score of 41.8 failures to react successfully. This gives an advantage to the combination method of 17.3 points.[1]

Fortunately also we can get some, though imperfect, indications as to the relative functional efficiency of the two methods from the beginning of the process. This is yielded as usual by the ability to react to the new cases at the first presentation of each "pack" of the evolution series. The result is shown in Table XIV. In the combination method, only "packs" II, IV and VI have any significance as to the progress of the conceiving process, for they alone are new cases. Such of the final averages as are significant, are shown graphically in Figure VII, where it is seen that the advantage of the combination method appears immediately and probably increases throughout the process. An apparent exception is seen in "pack" VI. There the errors by the combination method suddenly increase greatly and considerably exceed those by the simple-to-complex method, which itself has increased a certain amount normal to this "pack". Immediately afterward on the tests, the advantage is restored to the combination method. An inspection of the data in Table XIV shows that the tendency for the larger score on the sixth "pack" by the combination method was shared by the majority of the subjects. It is possible that this particular method of evolving concepts may be especially weak at this stage of its progress, when it comes to functioning on unusually difficult novel situations. In this connection it may be well to recall that owing to the technique employed (p. 19) it is impossible that the difference noted should be caused either by differences in difficulty of the material or by individual differences of the subjects in rate of learning.

The results of the experiment may be formulated as follows:

1. A combination of abstract presentations and concrete ex-

---

[1] A computation of the "unreliability" of this average shows that there are only about twelve chances in ten million that the "true" average would not show an advantage for the combination method.

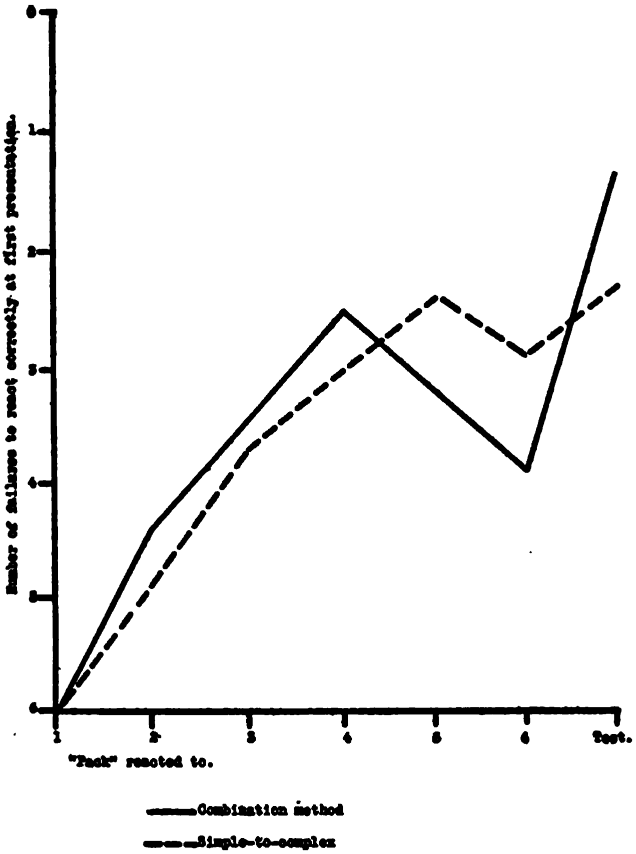


FIGURE VII.—Showing the origin of the advantage of the combination over the simple-to-complex method, evolution series, Experiment F.

amples is upon the whole distinctly more efficient in evolving functional concepts than either method alone.

2. The advantage of the combination method appears at the beginning of the process and probably continues to increase at a fairly uniform rate through the first two-thirds of the process, as far as it was carried in the present experiment.

3. It may be that at certain stages of perfection, concepts evolved by the combination method are less efficient in coping with new situations of exceptional difficulty than when evolved by the simple-to-complex method.

The above experiment has considered only one of an important group of problems: Is it more economical to make the abstract presentation at the beginning or end of the process? What is the relative efficiency of making the abstract presentation in the middle of the process, and having it both preceded and followed by concrete cases? Is it better to have the abstract presentation and the concrete cases alternate frequently as above, or to have the first half of the time devoted to one method and the second half to the other? Would it be better to have the abstract presentation come at the beginning or end of the process? What is the most economical distribution of time that could be devoted to the two methods in the combination?

Clearly the above experiment has only broached the problem of the economy of abstract presentation. Numerous other problems of a similar nature will at once suggest themselves. The explorational aim of the present study will have been attained if the general economy of the combination method is shown, and an adequate and convenient technique for the solution of the various subsidiary problems demonstrated.

VI

THE EFFICIENCY OF EVOLVING CONCEPTS FROM A SERIES OF CONCRETE CASES WHERE THE ATTENTION IS ATTRACTED TO THE COMMON ELEMENT *IN SITU*

Experiment G

Closely related to the method of concept evolution by abstract presentation, is that of demonstration. For our purpose demonstration consists essentially in the presentation of a series of concrete cases and directing the attention of the subject to the essential characteristic of each in its concrete setting. The problem before us then is: What is the relative efficiency of evolving functional concepts from concrete cases in which the attention of the subject is continuously attracted to the significant common element, as compared with the ordinary simple-to-complex method.

Only a slight modification of the technique of Experiment F was necessary in order to adapt it to the present problem. As there, *all twelve* of the evolution series were arranged in the simple-to-complex order with the simple end of the series at "pack" one. But with half of the evolution series, e.g., G to L, the characters were redrawn so the common element appeared in a saturated red, everything else being the same. The redrawing was done with great care and the new characters except for the color of the common element, were practically identical with the originals. It was assumed that the color would attract the attention of the subject mildly but continuously to the common element. Nothing whatever was said concerning the color however, the same instructions as usual being given. With half of the subjects the process was reversed, series A to F being given in the simple-to-complex order with the common element in red, and series G to L as normal simple-to-complex. Ten university students were used as subjects.

The tests reveal a decided advantage in favor of attracting attention to the significant common element *in situ.* The results of the tests are shown in Table XV. Nine of the ten subjects showed a more or less decided superiority where the common element was given in red while the average of all yields only 29 failures to react successfully out of a possible 108 as against 42.4 by the ordinary simple-to-complex method. This gives a net advantage of 13.4 points in favor of attracting attention to the common element in its concrete setting.[1] It does not appear to be quite so efficient as the alternate abstract presentation and concrete example (Experiment F) which yielded an advantage of over 17 points at almost exactly the same stage of the normal simple-to-complex process. But the significance of such small differences is not great where the variation is so considerable.

TABLE XV

Showing the relative number of failures to react correctly on the tests for the two methods compared, Experiment G.

		Number of failures to react on test out of possible 108.		
Sub-ject	Common element in red Series:	Common element in red	Simple-to-complex	Advantage of common element in red
Wen.	A to F	17	58	+41
Hal.	" " "	53	63	+10
Con.	" " "	13	41	+28
Mar.	" " "	51	61	+10
Moe.	" " "	23	27	+ 4
Bec.	G to L	27	42	+15
Col.	" " "	21	43	+22
Mel.	" " "	19	24	+ 5
Lin.	" " "	39	50	+11
McK.	" " "	27	15	—12
Average		29.0	42.4	+13.4
M. V.				10.5
S. D.				13.7

In this experiment we are again able to get some indication as to the relative functional efficiency of the two methods throughout the process from the beginning. This is furnished as before by the number of failures to react successfully to the evolution

---

[1] A computation of the "unreliability" of the average shows that there is only one chance in about one thousand that the "true" average would not show an advantage for attracting attention to the common element *in situ.*

TABLE XVI

Showing the relative efficiency of the respective methods in reacting to the evolution series at first presentation, Experiment G.

		Number of failures to react at first presentation of evolution series.											
		Common element in red						Simple-to-complex method					
Subject	Common element in red in Series:	I	II	III	IV	V	VI	I	II	III	IV	V	VI
Wen.	A to F	6	5	4	1	0	1	6	4	3	3	2	2
Hal.	" " "	6	6	5	2	2	2	6	3	4	5	2	3
Con.	" " "	6	2	2	0	0	2	6	4	1	2	0	3
War.	" " "	6	5	5	3	2	3	6	4	2	4	5	5
Moe.	" " "	6	3	2	3	1	1	6	4	3	3	2	2
Bec.	G to L	6	1	3	2	0	0	6	5	4	4	3	5
Col.	" " "	6	5	2	2	0	0	6	4	4	3	1	0
Mel.	" " "	6	3	3	0	1	0	6	5	4	3	1	4
Lin.	" " "	6	6	3	2	3	1	6	6	5	6	3	2
McK.	" " "	6	3	2	1	0	0	6	4	4	3	1	0
Average	....	6.0	3.9	3.1	1.6	.9	1.0	6.0	4.3	3.4	3.6	2.0	2.6

"packs" at their first presentation. These are shown in detail in Table XVI. The final averages by the two methods are shown graphically in Figure VIII. There it is evident at a glance that the advantage of having the attention of the subject attracted to the common element in its concrete setting appears at the very beginning of the process and continues to increase until the process is interrupted by the tests. A second important fact brought out by this figure is the shrinkage which takes place in the ability of the subjects to react to new cases when they pass over from where the common element is in red (pack VI) to the tests where there is no color to aid them. The shrinkage is not great but probably real. In fact it is exactly what one would expect. It recalls the marked discrepancy found between the ability to define (Experiment E, p. 43) and the ability to react to new cases, where the common element had been given outright. It probably results from a similar cause.

The results of the present experiment may be briefly formulated as follows:

1. There is a distinct advantage in evolving concepts from concrete cases in the simple-to-complex order where the attention of the subject is attracted to the essential common element *in situ*, over the normal simple-to-complex method.

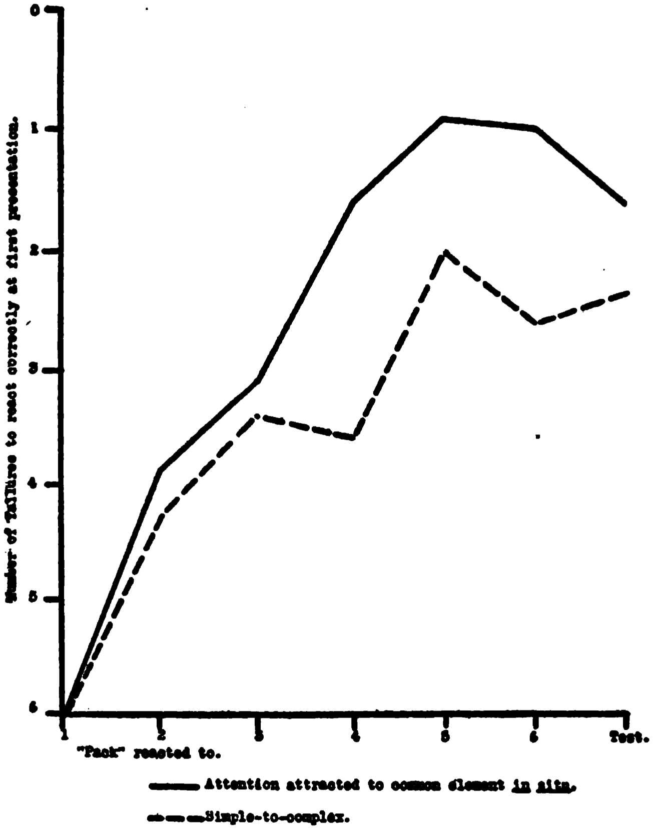


Figure VIII.—Showing the origin of the advantage of calling attention to the common element *in situ*, evolution series, Experiment G.

2. The advantage appears at the beginning of the process and increases continuously as far as it has been investigated.

3. There is a mild shrinkage in functional ability when the subject passes from the artificial material to normal experiences, but there still remains a substantial functional advantage in attracting attention to the common element while in its concrete setting.

As in previous chapters, the problem here investigated is only one of a group. What would be the effect of calling the subject's special attention to the red strokes as being the common element? Would the present method be improved by alternating it with normal concrete cases somewhat as in Experiment F? Would it be better in such a case to have the normal concrete case precede in the series, or follow, or both? Would the addition of the naked common element to the combination be an advantage or not? If so in what position would its advantage be greatest?

As pointed out at the end of the last chapter, the aim of the present study is attained if the general economy of the method is shown and an adequate technique indicated for the solution of the numerous problems of a related nature which it suggests.

VII

THE EFFICIENCY OF EVOLVING CONCEPTS AS A FUNCTION OF THE THOROUGHNESS OF FAMILIARITY WITH THE CONCRETE CASES

EXPERIMENT H

An important factor in all kinds of learning is the matter of thoroughness. Theorists have not been as specific in their treatment of this factor in relation to the evolution of concepts as upon some of the matters discussed in the previous chapters. There seems however to be quite a general belief that a thorough mastery of each concrete case involved in the evolutionary process will be specially conducive to efficiency in coping with new situations. This accordingly determines our problem: In the evolution of functional concepts, is it more economical to have an evolution series made up of six concrete cases each experienced moderately well, or to have half as many concrete cases each experienced twice as well?

Only a moderate variation of the general method used in the previous experiments was required in the present problem. The characters of the evolution series were rearranged so that all the "packs" were of presumably equal difficulty. This at the same time largely eliminated the simple-to-complex arrangement shown in Plate I. It was done in the following manner: If the numbers from one to six represent increasing complexities of the characters as arranged, e.g. in Series A, Plate I, the new arrangement may be conveniently represented in the following table:

	Pack I	Pack II	Pack III	Pack IV	Pack V	Pack VI
Series A	1	2	3	4	5	6
" B	2	3	4	5	6	1
" C	3	4	5	6	1	2
" D	4	5	6	1	2	3
" E	5	6	1	2	3	4
" F	6	1	2	3	4	5

A glance at the table shows that every "pack" contains one character of each of the six grades of complexity and that all are consequently of approximately equal difficulty. The other six evolution series (G to L), were rearranged in an exactly similar manner.

A second modification of the technique previously used was to make the characters in "packs" I, III and V do double service. The characters of each of these "packs" were learned as normally but also a second time in place of the characters normal to the "pack" following—II, IV and VI respectively. That is, the characters normal to "pack" I would be used both in "packs" I and II, characters normal to "pack" III would be used both in "packs" III and IV, characters normal to "pack" V would appear both in "packs" V and VI. The characters normal to "packs" II, IV, and VI of course did not appear at all. A uniform time of nine exposures was given to each "pack." Thus in evolution series G to L, 54 exposures were equally divided among six concrete cases while in series A to F, 54 exposures were equally divided among *three* concrete cases. In the first case 9 exposures were given to each concrete case which we shall regard as "moderate" familiarity, while in the second case 18 exposures or twice as many are given to each concrete case, which is regarded as "thorough" familiarity.

As usual in order to equalize any possible difference in the difficulty between the two groups of concept series, the process was reversed with half of the subjects. In this case G to L became the "thorough" series and A to F the "moderate" series. In order to equalize any difference in difficulty existent between the characters included in the "thorough" series and those excluded from it, with part of each group of subjects, the characters of "packs" II, IV and VI did double service, those of "packs" I, III, and V being excluded in this case.

The subjects in this experiment were scholarship students from the University High School. They ranged from 13 to 18 years of age.

The tests revealed a small but fairly consistent advantage in favor of the method of "moderate" familiarity. The results are

TABLE XVII

Showing the relative efficiency on the tests of the methods of evolving concepts respectively by "moderate" and "thorough" familiarity with the concrete cases, Experiment H.

			Failures to react on the tests out of a possible total of 108.		
Subject	"Thorough" method in:	In "thorough" method, characters used from "packs":	"Moderate" method	"Thorough" method	Advantage of "moderate"
Hug.	Series A to F	I, III, V	61	76	+15
McC.	" " " "	" " "	59	60	+ 1
Hay.	" " " "	" " "	95	94	− 1
H. S.	" " " "	II, IV, VI	88	74	−14
Cal.	" " " "	" " "	55	83	+28
Fit.	Series G to L	I, III, V	56	71	+15
Eps.	" " " "	" " "	68	72	+ 4
Pag.	" " " "	" " "	35	77	+42
L. S.	" " " "	II, IV, VI	28	69	+41
Don.	" " " "	" " "	77	89	+12
Average			62.2	76.5	+14.3[1]
M. V.					13.9
S. D.					17.3

[1] A computation of the "unreliability" of this average shows that there is only about one chance in 220 that the true advantage for the "moderate" method would be less than zero.

shown in detail in Table XVII. Eight of the ten subjects gave a more or less decided advantage for the "moderate" method. All the subjects taken together give for the "moderate" method an average of 62.2 failures to react correctly out of a possible 108, while the "thorough" method shows an average of 76.5 failures, a difference of 14.3 points in favor of the "moderate" method.

The conditions of the experiment permit us to secure a limited view of the origin of this difference throughout the evolution series. Table XVIII shows in detail the number of failures to react at the first presentation of the various "packs." It will of course be noted that the scores in "packs" II, IV and VI of the "thorough" series are not significant because each character involved has been experienced nine times in the previous "pack" though in a different order. This accounts for the extremely small number of failures in each of these "packs." Their non-significant nature is therefore indicated by inclosing them in parentheses.

The final significant averages of the two methods are shown graphically in Figure IX. It is evident that the advantage of the "moderate" method is not a temporary matter but dates from the very beginning of the process. A second feature of interest in Figure IX is the fact that the lower or thorough curve shows almost no rise whatever at "pack" III after a total of eighteen exposures. This is much less than might be expected from only nine or half as many exposures. It suggests that at this stage of the process, the additional nine exposures did more harm than good, at least in terms of immediate functional efficiency.

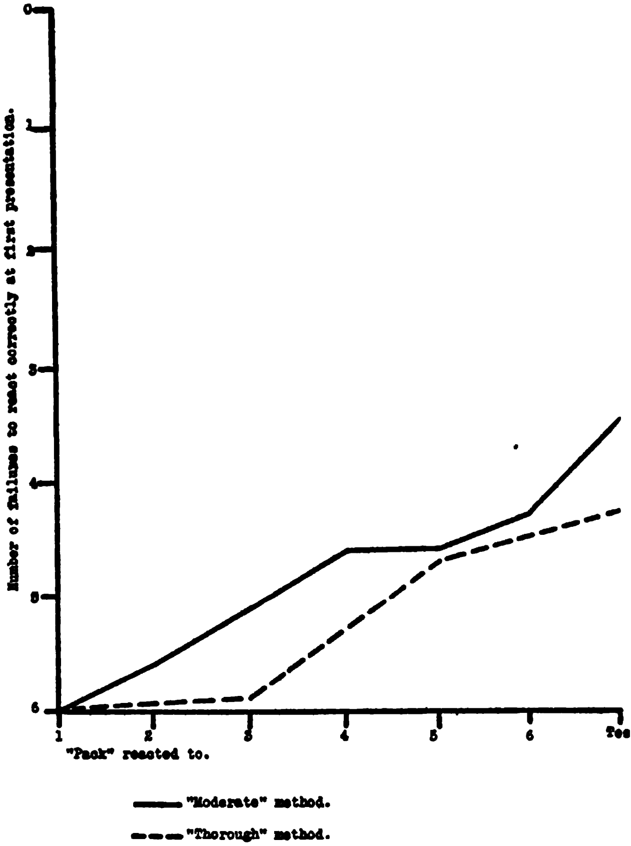


Figure IX.—Showing the origin of the advantage of the "moderate" method over the "thorough" method, evolution series, Experiment H.

The results of Experiment H may be summarized as follows:

1. A moderate amount of time spent upon each of six concrete cases is perceptibly more efficient in the evolution of functional concepts than twice as much time spent upon each of three concrete cases.

TABLE XVIII

Showing the comparative functional efficiency of "moderate" vs. "thorough" familiarity in reacting to the "packs" of the evolution series at first presentation, Experiment H.

		The number of failures to react correctly at first presentation of "packs".											
	"Thorough" method	"Moderate" method						"Thorough" method					
Subject	in Series:	I	II	III	IV	V	VI	I	II	III	IV	V	VI
Hug.	A to F	6	5	3	6	6	4	6	(2)	6	(4)	6	(1)
McC.	" " "	6	6	6	5	5	5	6	(4)	6	(0)	3	(0)
Hag.	" " "	6	6	6	5	6	6	6	(5)	6	(2)	5	(4)
H. S.	" " "	6	6	6	5	5	5	6	(5)	6	(1)	6	(0)
Cal.	" " "	6	6	4	2	3	4	6	(3)	6	(0)	5	(1)
Fit.	G to L	6	6	5	4	4	4	6	(5)	6	(1)	5	(2)
Eps.	" " "	6	5	5	5	5	5	6	(4)	5	(1)	5	(1)
Pag.	" " "	6	6	5	4	4	4	6	(3)	6	(2)	4	(1)
L. S.	" " "	6	4	5	4	3	2	6	(3)	6	(0)	4	(0)
Dan.	" " "	6	6	6	6	5	4	6	(4)	6	(4)	4	(1)
Average	....	6.0	5.6	5.1	4.6	4.6	4.3	6.0	(3.8)	5.9	(1.5)	4.7	(1.1)

2. This superiority appears at the outset and is maintained at least through the first third of the process, as far as investigated in the present experiment.

One or two critical observations may be made at this point. In varying the thoroughness of familiarity, we have also varied the number of cases. Does this introduce the uncertainty of interpretation due to two variables? While interesting from the standpoint of logical theory, the objection is of no practical importance because the two "variables" are so rigidly related that they act essentially as a single factor.

Of more importance is the question as to why the "thorough" method should prove less efficient? The reason seems to be that the eighteen exposures of the "thorough" method carried the process of generalizing abstraction as regards the individual concrete example, farther into the region of "diminishing returns" than the nine exposures of the "moderate" method. This raises the farther question as to how early in the experience with a given concrete example, the period of "diminishing returns" sets in. Or more generally, what is the curve of the contribution of a single concrete example to generalizing abstraction, in terms of time? The writer ventures a guess that it may rise rapidly to a maximum very early after which there may be a protracted period of "diminishing returns" which may even pass into a negative value. No doubt this may differ with different material and possibly with different individuals. If the maximum is reached before the ninth exposure, the "moderate" method will triumph. If it is reached during the second nine exposures, the "thorough" method must win. This suggests a number of important problems for future investigation.

VIII

THE EVOLUTION OF FUNCTIONAL CONCEPTS AS RELATED TO THREE TYPES OF ABNORMAL MENTALITY

EXPERIMENT I

In the search for quantitative individual differences in the mode of evolving concepts, two general lines of procedure are open. One is to compare the performances of various normal individuals with one another and note the differences. A second way is to compare the performances of minds known to be characteristically different from the normal and from one another. The latter method was adopted in the present experiment as most likely to reveal striking individual differences if such exist. It was hoped that an experiment of this kind in which several of the higher mental processes could be conveniently measured without seriously disturbing the others, might throw some light both upon the process of generalizing abstraction on the one hand, and upon the state of some of the higher mental processes among characteristic types of insanity, on the other. The types of mental abnormality selected for the purpose were (1) constitutional inferiority, (2) dementia precox and (3) paresis.

The method employed requires little comment. It was essentially the same as that used in Experiment A (p. 9 ff.). The only exception was that in order to secure incidentally a measure of the power of retention, one week was interposed between perfecting the reaction to "pack" I and beginning work on "pack" II. At the end of the week and just before beginning work on "pack" II, a test was made to see how many of the characters of "pack" I were still retained. After this they were relearned until the score had reached the same excellence as when work was stopped a week previously i.e., a perfect score on two successive revolutions of the drum. This was done both to secure an additional measure of the retentiveness and to bring the mind into a state approximately similar to that when the work was interrupted a week previously.

Eight subjects were used in the experiment. They consisted of three constitutional inferiors, three dementia precox and two paretics.[1] They were all patients of the Wisconsin State Hospital for the Insane, at Mendota, Wisconsin. They were very carefully selected from a group of over eighty individuals who had been previously experimented on at some length in an effort to adapt the Binet-Simon tests to the determination of the extent of intellectual degeneration among the insane. The eight selected from the larger group for the present experiment were chosen upon a joint basis of tractability under experimental conditions, of furnishing relatively clear types of their respective diseases, and of an approximately equal intellectual level. The latter was determined by the very careful mental tests to which they had been subjected. The level chosen was roughly the ability to pass from thirty-five to forty of the Binet tests (Goddard's revision). Upon trial it was found impossible for paretics of this grade of general intelligence to learn the reactions required by the experiment, so a new group of paretics was secured having an intellectual ability sufficient to pass from forty-eight to fifty of the tests.[2]

The patients had all become more or less accustomed to examinations from those ordinarily given by the hospital physicians. Through having received the Binet tests, they had also become fairly accustomed to the writer in an experimental capacity. He frequently met them in the wards and in general cultivated easy friendly relations with them. All appeared to coöperate very willingly in the experiment and showed a distinct interest in its progress. The experiments were carried out in an ordinary patient's room in one of the quietest wards of the hospital. The size and lighting of the room and the position of the apparatus were very similar to those of the psychological laboratory previously described (p. 21). Especially frequent rest periods were given the subjects to prevent fatigue.

---

[1] Brief case histories of these patients are given in Amer. J. of Psychol., Vol. XXVIII, pp. 419-435. A third paretic was included in the original plan, but he proved to be a poor subject and was finally excused.

[2] 51 was found to be the median number of tests passed by normal adults of about the same age and station of life. The so-called "wide range" method of testing was employed.

The results in general show the insane to be distinctly inferior to normals in the formation of associations and in generalizing abstraction, but apparently fully equal in retentiveness of association once they are formed. The data concerning the formation and retention of associations with these subjects has been fully discussed elsewhere (*3*).[3] The performance of the abnormals on the tests is shown in Table XIX. The normal subjects with whom they may be compared most appropriately are those of Experiment A. The corresponding data for these latter subjects has been given (though in a divided form) in Table I (p. 23). The average of all the abnormals is 127.4 failures to react out of a possible 216 as against 74.3 for the normal subjects. Two abnormal subjects reach the mean of the normals but no normal subject does as badly as the mean of the abnormal subjects.

The superiority of the normals appears much more striking when the amount of time required to produce the respective scores is taken into consideration. This is shown in detail for the abnormal subjects in Table XX. The final averages for the normals of Experiment A have already been given on p. 30. The two sets of averages are shown graphically in Figure X. There

TABLE XIX

Showing the grade of intelligence and the number of failures to react successfully on the tests, out of a possible 216, Abnormal subjects, Experiment I.

Subject	Type of abnormality	No. Binet tests passed	No. failures to react successfully on tests
Bur.	const. inferior	37	73
Sch.	" "	38	146
Syg.	" "	35	135
Don.	dementia precox	37.5	118
Jack.	" "	40	130
Jaco.	" "	47.5	75
Har.	paresis	48	159
Sau.	"	50	183
Average			127.4

---

[3] In addition, the fluctuations at the threshold of recall was found to be about twice as profound with the insane as with normals.

it appears at a glance that the time consumed by the abnormals particularly in perfecting the reaction to "pack" I is many times that of the normals. The total time spent by the abnormals is nearly four times as great as that spent by the normals. The single figure expressing the ultimate efficiency of the two groups

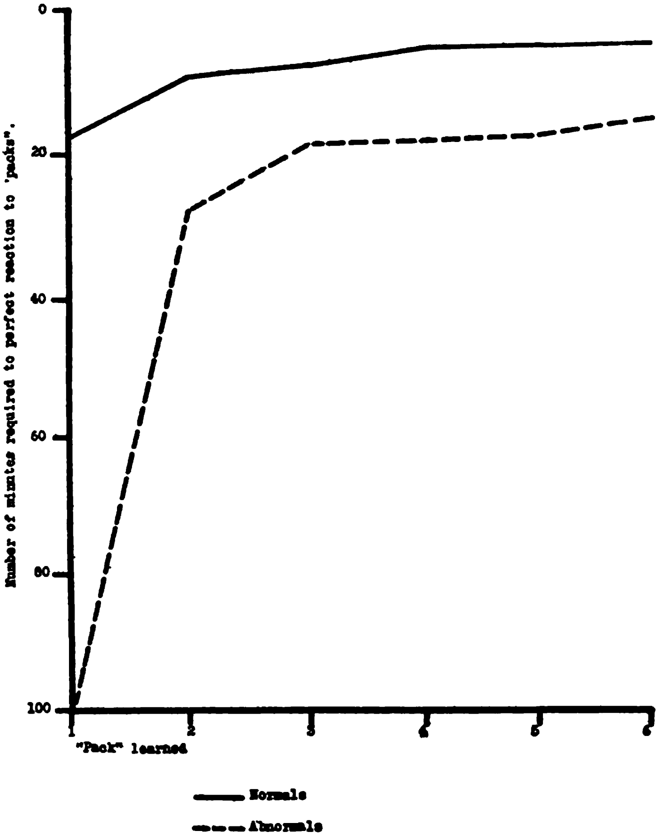


FIGURE X.—Showing the number of minutes required by the normals and the abnormals respectively in perfecting the various reactions, evolution series, Experiment I.

of subjects is the number of successful reactions on the tests per unit time spent on the evolution series. It is:

Abnormals	.45	successes per minute spent.
Normals	2.81	" " " "

or over six times as great for the normals as the abnormals.

Disregarding the differences in amount of time consumed, the number of failures to react successfully at the first presentation of the successive evolution "packs" offers us a convenient indication of the origin of the differences in efficiency found between the two groups of subjects. These data for the abnormals is given in Table XXI. Corresponding data for the normal subjects of Experiment A are given in Table II though in a slightly different form. The corresponding averages for the normals computed from Table II are:

TABLE XX

Showing the number of minutes required by each abnormal subject to perfect the reaction to each of the "packs" of the evolution series.

	Minutes per "pack"					
Subject	I	II	III	IV	V	VI
Bur.	89	29	7	7	9	14
Sch.	170	38	17	17	16	18
Syg.	72	18	11	13	7	12
Don.	45	12	7	7	11	8
Jack.	52	31	43	34	15	14
Jaco.	39	15	8	7	9	7
Har.	200	42	28	25	27	19
Sau.	150	32	23	25	41	22
Average	102	27	18	17	17	14.3

TABLE XXI

Showing the number of failures to react successfully at the first presentation of the evolution "packs," Abnormal subjects, Experiment I.

Subject	"pack" I	"pack" II	"pack" III	"pack" IV	"pack" V	"pack" VI
Bur.	12.0	12.0	4.5	3.0	4.5	4.0
Sch.	12.0	10.0	8.5	10.0	8.0	10.0
Syg.	12.0	10.5	9.0	9.5	8.0	9.5
Don.	12.0	8.0	7.0	8.0	5.5	6.0
Jack.	12.0	11.0	10.0	8.0	10.5	8.5
Jaco.	12.0	7.0	7.0	5.5	4.5	5.0
Har.	12.0	10.5	11.0	9.0	9.5	9.0
Sau.	12.0	12.0	12.0	11.0	12.0	10.0
Average.	12.0	10.1	8.6	8.0	7.75	7.6

"pack" I	"pack" II	"pack" III	"pack" IV	"pack" V	"pack" VI	test
12	8.7	7.6	6.3	5.5	5.0	4.1

The two sets of averages are shown in Figure XI. From these curves it is evident that the abnormals follow the same general

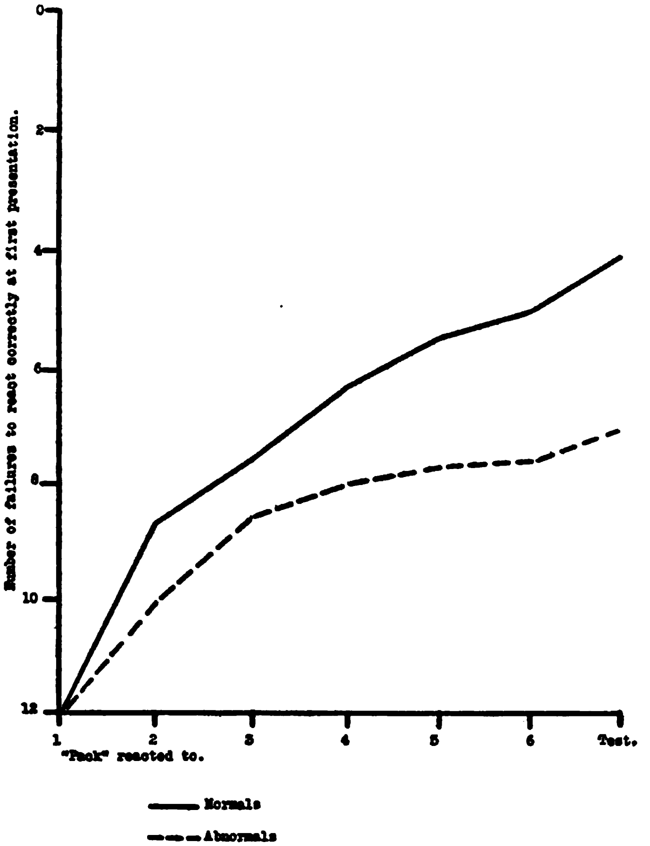


FIGURE XI.—Showing the origin of the advantage of the normals over the abnormals, evolution series, Experiment I.

course of improvement as the normals but from the very beginning there is a continuous and uniform divergence of the two curves, the normals steadily outstripping the insane.

At the conclusion of the tests the insane subjects were requested to draw on a suitable blank their idea of what a character must be in order to be called by the various names. Only a few scrawls were obtained. In general there appeared an almost total inability to define in this sense. Results obtained from normals in the previous experiments indicate in a general way that this may be one of the most striking and significant differences existing between normals and the "insane." No particular difference in this respect was discernible among the various types of abnormals included in the present study.

The results of the present experiment may be briefly formulated as follows:

1. Constitutional inferiors, dementia precox and paretics show a great disturbance in the power to form association. This disturbance is distinctly more marked with the paretics than with the other two groups.

2. Once associations are formed, however, the three types of abnormals appear to retain them as well as normals.

3. The average rate of evolving functional concepts is only about one-sixth as rapid for the abnormal group as for the normal group. The advantage of the normals appears at the beginning of the process and increases continuously as far as the experiment carries it.

4. There is an almost total lack in the abnormal group of ability to define the concept by drawing, whereas the normals ordinarily show considerable ability at about the same stage of the evolutionary process.

Of the above four conclusions, the first probably depends largely on faulty attention. If one may judge from the actions of the subjects however, there was no lack of attention in the sense of effort or will to attend. The second probably reflects a physiological condition of stability of the synaptic connections. The slow rate of evolving concepts is probably largely a function of the slow rate of forming associations though this does not account for all of it, since the disturbance is considerably greater in the former than the latter. The practical inability to define concepts which have a proven functional efficiency as contrasted with normal subjects, may be related to the great role which the subconscious plays in the abnormal mind. This last point offers perhaps the most fruitful suggestion for a future investigation.

IX

THE RELATIVE RATE OF EVOLUTION OF FUNCTIONAL CONCEPTS AT THE SEVERAL STAGES OF THE EVOLUTIONARY PROCESS

Practice-curves for various kinds of learning have long occupied the attention of experimental psychologists. The form characteristic of ordinary associative learning is well known (*5*, 575). Relatively little is known however of the characteristic course of the more complex forms of learning. A recent writer indeed found it necessary to conjecture as to the probable form of the curve for analytical learning (*15*, II, 344). It therefore becomes desirable to determine the curve for generalizing abstraction as one of the most characteristic of the more complex learning-processes. A number of lines of evidence converge upon this problem.

In the previous chapters we have shown numerous curves which purport to represent quantitatively the course of generalizing abstraction. It is obvious that for our present purpose the curve must be plotted strictly on a basis of time spent in the process. None but Figures V and VI fulfil these conditions. Owing to the peculiarities of the material employed, a composite curve from each figure suits our present purpose better. The two curves thus obtained are shown in Figure XII. In addition the curves from Figures I and III may be reduced to approximately similar conditions, in which case they resemble very closely those shown in Figure XII. So far as they go, these curves present no obvious difference from the conventional practice curve for ordinary associative learning.

Experiment J

A preliminary experiment designed only partly for the present purpose has an indirect bearing upon the problem. The characters in the column marked "Evolution Series" in Plate III were

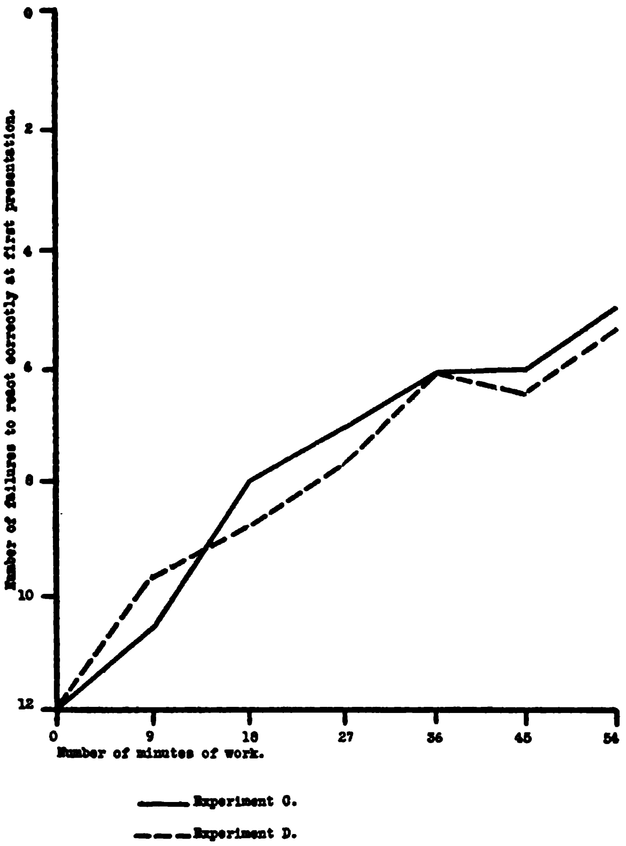


FIGURE XII.—Curves of generalizing abstraction, evolution series, Experiments C and D.

exposed to subjects in an endless series for periods of two seconds each with a blank period of two seconds between exposures. The subject was directed to find the strokes common to the various characters as quickly as possible. When he thought he had found anything, the apparatus was stopped upon the following blank space, and the subject drew what he believed he had found. The exposures were then resumed until he thought he had made another discovery when a new and more perfect drawing was made. Record was made of the number of exposures between successive drawings. A simple practice-experiment was given as a preliminary, the results of which were discarded. Sixteen Normal School students acted as subjects.

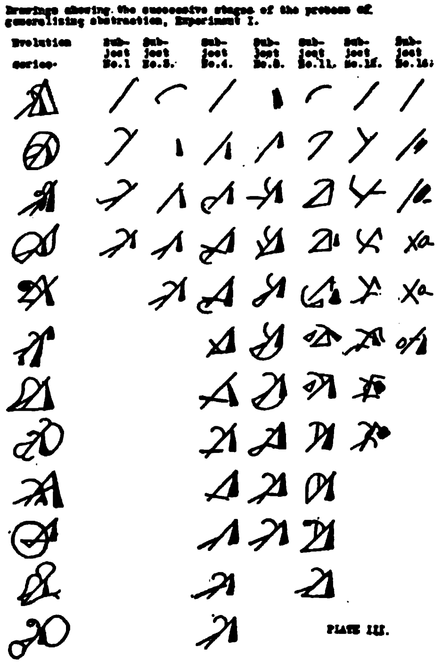


PLATE III.

Characteristic results of the experiment are shown in Plate III. The average of all the subjects shows that it took 14 exposures to make the first discovery, 45 more to make the second discovery, 111 more for the third and 187 more for the fourth. Clearly a longer and longer time is required for each successive discovery and the increase follows a very definite law. If the successive discoveries be assumed to have equal increments of difficulty, we may obtain from these figures a kind of curve of generalizing abstraction (Figure XIII).[1]

That this systematic increase in the time required to discover the successive strokes is primarily an expression of the mind doing the abstracting rather than of the difficulty of the characters, is shown by the fact that the characters are of a fairly uniform

---

[1] This curve is in no strict sense comparable with those of Figures XII and XIV.

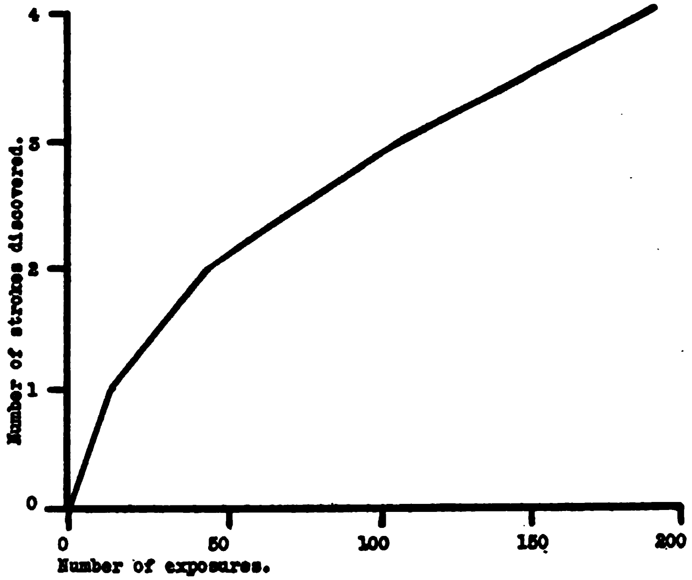


FIGURE XIII.—Curve of generalizing abstraction by the method of Experiment J.

gradation in difficulty. This in turn is shown by the distribution of variation in their order of discovery. (Table XXIII.)

TABLE XXIII

Showing the number of times that each stroke was discovered first, second, third and fourth respectively.

	1st.	2nd.	3rd.	4th.
Long diagonal stroke	10	6	0	0
Shaded stroke	4	6	3	3
Curved stroke	2	4	6	4
Short diagonal stroke	0	0	7	9

Moreover it is also clear from Table XXIII that the short diagonal stroke is, upon the whole, the most difficult one. It so happens that seven subjects discovered this stroke third, and discovered either the second or the third stroke last. Here then we have the inherent order of difficulty operating *against* the tendency in question. Yet these seven subjects give the following averages:

10, 22, 53, 86.

The tendency is still present showing clearly that the increased time required for each succeeding discovery is a function of the mind making the discovery.

EXPERIMENT K

The inadequacies of the above methods of securing a curve of generalizing abstraction are numerous. Experiment J obviously does not yield a functional measure. In addition it suffers from most of the disadvantages urged against the method of Fisher (p. 7). The method of Experiments A and D suffer from a distortion of the score owing to certain inequalities of the characters of the various "packs," the long intervals between measures, the small number of measures at each interval, the unfinished state of the curve and the necessary combination of many individual records into a single composite curve. To remedy these defects a new experiment was set up which involved the construction of an entirely new apparatus.

The new apparatus was built very much after the plan of the one used in the previous experiments. Instead of holding a small number of cards, the drum bore a long band which hung suspended from it and upon this an indefinitely large number of characters could be attached. The band of white oilcloth, two inches wide, was placed with the rough side toward the subject. Small spurs on one edge of the drum engaged eyelets in the band to insure accurate movements. The characters of Plate I were photographed, printed on a matt surface in exactly the same size as the originals and glued to the band at proper intervals. They were put on by "packs" in the order I to XII. The order of characters within each "pack" was determined by chance except that two characters by the same name should not be too close together at the joining of two consecutive "packs." The 144 characters required a band over twelve feet long. The apparatus was elevated so that the band when joined at the ends and suspended from the drum, just cleared the floor. A screen of gray cardboard was built in front of it. This had a small square hole cut just on a level with the subject's eyes so as to expose a single character. Suitable guides were provided to keep the characters close to the aperature and to prevent vibrations while being viewed. The shifts from one character to the next were practically instantaneous as in the previous experiments and the exposures were likewise five seconds each.

The subject was instructed to learn as quickly as possible the names of the characters which would appear at the "window," so that he could pronounce them at sight. He was told plainly that there were several "families" of characters, that the members of each "family" bore a certain family resemblance and also bore the same name.[2] It was explained that he would be prompted in the middle of each exposure, that he was to repeat the name when prompted, and that he was to attempt to name the characters for himself as soon as he had any indication as to what the name might be. It was pointed out that errors were not considered in the score but success only, and that on the long run reactions to even faint suggestions would materially improve the score. The results were recorded much as in the previous experiments only on a large special blank prepared for the purpose.

By the above technique, every one of the 144 characters was seen before the first was exposed a second time. This consumed twelve minutes of vigorous mental activity. It thus came about that when the characters began returning they were not recognized individually at all. Consequently very little or no learning of the individual characters took place and the improvement in the score was due almost entirely to the progress of generalizing abstraction. By taking the average score for a period of twelve minutes we have 144 judgments which gives us a very reliable indication of the status of the process for the period. The difficulty of the material from point to point on the curve is not only uniform but identical. We may carry the process to any desired degree of perfection. And we not only can secure a reliable curve of generalizing abstraction from a single person but also for any single one of the twelve individual concepts evolved by that person.

Eight subjects in all were used in the various forms of this experiment. Their records appear in Table XXIV. The first

---

[2] We here departed from our policy of avoiding deliberate abstraction in order to escape any unnecessary delay at the beginning of the learning which might of itself produce the "initial plateau," to be mentioned presently.

TABLE XXIV

Showing the average number of successful reactions per "pack" for each successive period, Experiment K.

Subject	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI
Cha.	1/6	1 3/4	4 1/6	6 1/3	8	9 1/4	10	10 5/12	10 5/6	11	10 5/6	11 1/4				
Bes.	1/6	1 1/4	4	7 5/12	8	10 1/2	10 2/3	10 3/4	10 11/12	11 1/6	11 1/6	11 5/12	11 7/12			
Lap.	1/6	1	1 1/6	5/6	2/3	1	1 1/3	1 1/3	2 7/12	3	5 7/12	6 7/12	7 1/4	8 1/2	8 2/3	9
Kin.	1/3	1	2 2/3	5 1/3	8 1/3	8 5/6	10 1/3	11 2/3	11 5/6	12						
Pil.	1/6	1 1/6	3 1/2	4	6 5/6	8 1/3	8 5/6	9 1/2	9 1/6	9						
Wil.	1/6	5/6	5 1/3	7 1/2	10	11	10 1/2	11 2/3	11 2/3	11 2/3						
Coa.	2/3	1 1/6	2 1/4	4 1/6	6 2/3	9 1/6	8 5/6	9 1/2	9 5/6	9 5/6	10 2/3	10 1/2	10 2/3			
Pea.	1/6	1/3	2/3	2 1/3	2 1/3	3 1/3	5	5 1/3	6 1/2	7 1/6	8 1/6					

four subjects did the experiment exactly as described above. Subject Pil. did it the same except that drawings and introspections were taken at frequent intervals. Subject Wil. had a practice experiment on some other material of the same type before making the present record. The last two subjects (Coa. and Pea.), did the experiment using only the first 72 of the characters used with the others, i.e. only the characters of "packs" I to VI. In this case the characters were somewhat smaller than the originals, they were glossy, and the subjects were not told anything about the "family" resemblances. The results for the first three subjects are by 12-minute periods, for the rest by six-minute periods. Three characteristic curves from this table are shown in Figure XIV.

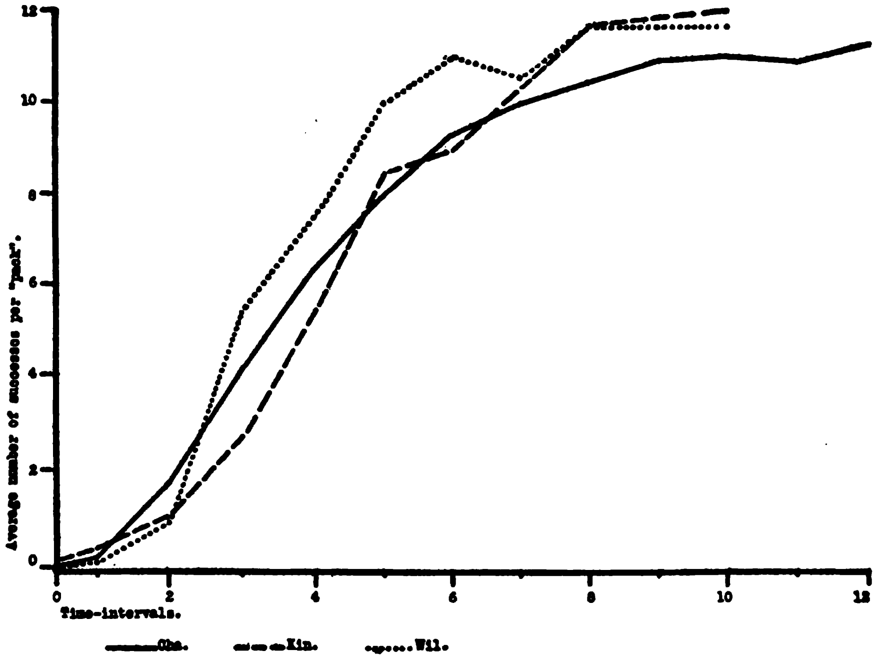


FIGURE XIV.—Showing typical (composite) curves of generalizing abstraction, Experiment K. Time-intervals for subject Cha. were twelve minutes; those for the others, six minutes.

Even to a casual observation these curves present a marked contrast to the curves hitherto examined, as well as to the conventional curves of learning. Without exception all eight subjects begin with a period of slow learning which increases rapidly until at an efficiency near one-fourth it reaches a maximum. After this its rate gradually decreases until as perfection is approached its advance becomes very slow indeed. It is also interesting to note what a variety of conditions yields this type of curve. It seems to matter not whether the subject knows at the beginning that he must isolate certain common elements, whether there has been preliminary practice, whether definitions are required, whether learning is fast or slow, or whether the curve is plotted by six or twelve minute periods. There is in all cases a brief initial plateau, then a rapid rise, followed by a protracted period of slower and slower learning.

But the ultimate curve of generalizing abstraction has not yet been reached. This is the curve of the evolution of a *single* concept. As suggested above, the flexibility of our technique enables us to secure this with ease by retabulating the data from which the figures of Table XXIV were secured. It will be remembered that the latter are really composite results from twelve individual concepts. The curves of individual concepts thus obtained, while showing characteristic differences from any found thus far, agree in the main with the curves which they make in combination. They differ in the more bold and striking manner of their emphasis of the peculiarities noted in the latter. In general the initial plateau tends to be more absolute, and when the break comes it is apt to be more abrupt. The period of diminishing returns is usually present but not in every case. Typical results from such a retabulation of the data for subject Cha. are shown in Figure XV (cf. the corresponding composite curve for this subject, Figure XIV).

CONCLUSIONS

We may formulate the results of our investigation of the curve of generalizing abstraction as follows:

1. The average functional improvement of a group of relatively homogeneous concepts all of which are evolved simultaneously, begins with an initial plateau or brief period of positive

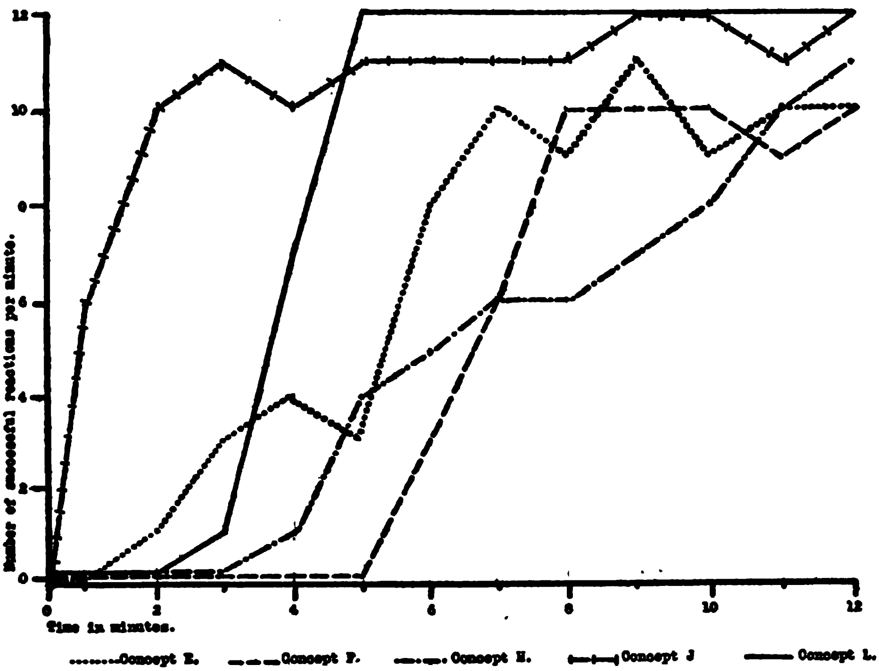


FIGURE XV.—Showing typical curves of the evolution of individual concepts, subject Cha., Experiment K.

acceleration. The rate of improvement tends to reach its maximum near the end of the first fourth of the process. Following this comes a more or less protracted period of negative acceleration which is terminated by the arrival at maximal efficiency of the concepts.

2. The functional improvement of the *individual* concept follows the same general course as the composite curve. The initial plateau is apt to be somewhat more protracted and absolute and the change from this to a period of rapid improvement tends to be much more abrupt (*13*, 51; *15*, II, 342 ff). The period of negative acceleration may be much abbreviated or entirely lacking, though with material of considerable difficulty it may be greatly extended.

The above statements clearly apply only to concepts evolved under conditions very similar to those of Experiment K. All methods agree as to the concluding period of diminishing returns. It is impossible to tell whether the initial plateau exists under the conditions of Experiments A to D. It probably could not appear in the results there even if it did, owing among other things to the fact that no point is located on the curve soon enough to show it. While both experiments duplicate important natural life processes, they are sufficiently different to make it unnecessary to assume the presence of the initial plateau in the latter process even if invariably present in the former.

The relative novelty of the initial plateau among learning curves makes it of interest to speculate as to its cause. The most probable explanation seems to be that in the compound process of generalizing-abstraction (*12*, 8) the abstraction (seeing the object in parts) must take place to a certain extent before generalization can begin[3] and that the score reflects only the progress of the latter process. Trial and error would also undoubtedly contribute to the same end.

The period of diminishing returns probably results from a multiplicity of causes. One is that chance hits upon significant elements must grow progressively less as the undiscovered significant elements grow less. A second factor is probably that when a concept is evolved to the extent of affording means for coping with some of the new situations presenting themselves, effort will largely cease on the successful occasions, which will progressively diminish the total amount of effort given to the process.

---

[3] This is indicated by a number of introspections obtained at the end of the process in our experiments. See Moore, op. cit., p. 120.

X

QUALITATIVE ASPECTS OF THE EVOLUTION OF CONCEPTS

Experiment L

We have already seen as an incidental product of Experiment I, (Plate III), what might be called serial cross sections of the concept throughout its process of evolution. The limitations of that form of experimentation have been pointed out. It was thought desirable therefore to secure similar cross sectional views from a more normal process of concept evolution. A very convenient means was immediately at hand in the technique used in the experiments described in the past chapters, particularly Experiments B and K.

In the first part of the present experiment the general technique of Experiment B was followed. But instead of limiting the evolution series to six "packs" as previously, it was extended to include all twelve "packs" (Plate I). At the conclusion of the learning of the second "pack," the subject was given a blank containing the names of the various concepts and asked to draw with as much care as possible, the strokes which a character must have in order to be called by the respective names. Four university students served as subjects in this part of the experiment. A typical series of such drawings are shown in Plate IV. Two other subjects repeated the experiment according to the general technique of Experiment K. They were called on for drawings usually every six minutes throughout the process. No difference was discernable between the drawings obtained by the two latter methods despite the differences in the form of their respective curves of learning.

In examining the drawings obtained for all three methods perhaps the most striking characteristic is the extremely gradual way in which the concepts come into existence. This is particularly true of the concepts evolved by the more natural methods (Plate

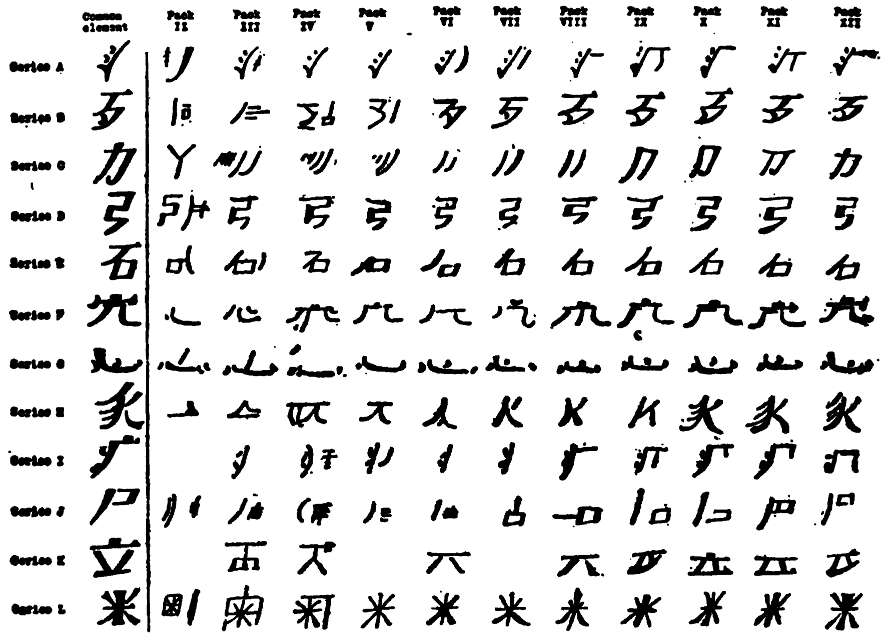


PLATE IV.—Showing serial cross sections of the evolution of twelve concepts in parallel, Experiment L.

IV). It is true that in some cases (Series D) where the common element is very simple, it may be almost or quite perfect at its first appearance. In such cases however it is apt to be accompanied by certain excess or non-essential characteristics. But again where the common element is seemingly very simple it may come to consciousness only in the most halting and gradual manner (Series C). The evolution of a rather complicated concept is shown in Series F and perhaps even better, by Series H.

A second interesting characteristic shown by this investigation is a tendency to transmute an essentially erroneous first notion of a concept by almost imperceptible degrees into its true form. This also is admirably illustrated by Series H, and many other examples are scattered through the records of the various subjects. The subsequent transformations are not unlike the transformations which the common philosophical and religious concepts have undergone in the history of thought.

The third and most important fact which emerges from the examination of the cross-sectional views of the concepts in the course of evolution is the enormous role of "trial and error" in the process (9, 80). This appears clearly by all three methods. It is shown in an objective way by the continual appearance of false strokes which disappear again after a more or less protracted existence. This objective evidence is confirmed in a certain sense by introspective accounts given by the subjects at the conclusion of the process. The present writer is strongly of the opinion however, that this *conscious* trial and error so revealed is largely spurious. It is believed that this deliberate seeking of the common element which appears in a gross way in the work of Moore, Grünbaum, Fisher, in our own Experiment J, somewhat less in Experiment K and to a slight extent in the other experiments, is largely an artifact resulting from experimental technique, so far as the *spontaneous* process of generalizing abstraction is concerned. The following brief analysis of certain inner aspects of the process may serve to make clear our notion of the real nature of the trial and error element.

1. Let us suppose that we have a concept evolution series in which each case is experienced until the reaction is perfected before passing on to the next. Let the various characteristics of the first situation be represented by the letters *a, b, c, d* and the required reaction by *m*. Now in perfecting the reaction, associative bonds must be set up between *m* and the situation *a b c d*. Certain parts of the situation will become focal in consciousness because they have greater attention values than other parts. Let *a* be the focal characteristic, *b* the secondary characteristic, *c* and *d* marginal characteristics. Accordingly a major bond will extend from *a* to *m*, a secondary bond from *b* to *m*, and minimal bonds from *c* to *m* and from *d* to *m*. But since there is nothing to indicate which characteristics of the situation are the really significant ones, the distribution of the various bonds must be largely a matter of chance.

2. At the next situation which requires the reaction *m*, the result will vary greatly according to whether the significant element in the *m*-series, turns out to be *a, b* or *c*. Suppose it is *a*. Then we may represent the new case by *a e f g*. In this case

the major bond from *a* to *m* will function, the reaction *m* will take place more or less promptly, and the bond from *a* to *m* will be strengthened by use. The process in this case is most simple. The chance trial has proven a success.

3. The opposite extreme is found where the significant element of the *m*-series turns out to be *c*. In this case we may represent the new situation by *c e f g*. The bond from *c* to *m* is subliminal. Associations must therefore be established between *m* and *c e f g* before the reaction can be perfected. No doubt the process will be somewhat complicated by the previous set of major bonds leading to *m*, which may be expected to have certain inhibiting influences upon the formation of other bonds (5, 580). But as before the bonds when finally formed may be expected to consist of one major bond and other bonds of rapidly decreasing strengths. Again also the attention values of the various characteristics of the new situation will be the dominating factors in determining the strength of the various bonds. But will the attention values be purely chance in this second case? It seems likely that they will not be entirely so. The bond from *c* to *m* even though subliminal may reasonably be expected to have a certain amount of tendency to attract attention to *c*. Obviously the stronger the original bond between *c* and *m*, the greater will be this tendency. Such an action if real, would greatly facilitate the process.

If the bond between *c* and *m* in the second case is still subliminal after the reaction is perfected, the process must be repeated until the subliminal bonds accumulate and become liminal, providing chance does not step in to suddenly shorten the process by putting *c* into a position of special attentional advantage.

4. Once more let us suppose that *c* is the significant element of the *m*-series in the first situation, and that the second situation is *a o h i*, *o* being here the significant element and requiring the *p*-reaction. The major bond *a*→*m* formed in the first situation will produce the false reaction *m*, with attendant unpleasantness and tendency to weaken the bond. In case a bond *o*→*p* has also been previously formed, a rivalry will ensue between the reactions *m* and *p*, with strong tendencies to mutual inhibition. The ex-

perimental results indicate that often a prolonged alternation of the two reactions will develop, the *m*-reaction each time resulting in unpleasantness. At length it will give place to the *p*-reaction, though often with unexpected relapses.

5. A third situation appears where the major bond is still between *m* and the irrelevant *a*, and where the new case contains both *a* and *c*, as *a c e f*. Here *a* will produce the reaction *m*, *accidentally* correct, and the spurious bond from *a* to *m* will be strengthened by use as in (2). Sooner or later it must of course encounter situations like (3) and (4) in which the *a*→*m* bond must be destroyed. The difficulties and inhibitions there described presumably will then take place in an aggravated form.

There thus appears a large element of trial and error in the process but not in the conscious or deliberate sense that this expression is often understood.

XI

SUMMARY

We may now briefly summarize the results obtained by the various experiments described in the preceding pages.

1. Evolving concepts by proceeding from the simple to the complex in the evolution series yields a substantial advantage over proceeding from the complex to the simple, where the activity in each succeeding case is continued until the reaction is just established. But by this method much more time is spent upon the simple cases of the simple-to-complex method than upon the corresponding cases of the complex-to-simple method. When the disproportion is partially reduced, the advantage of the simple-to-complex method is somewhat decreased; and when the disproportion is completely eliminated, the advantage of the simple-to-complex method is completely eliminated also. This fact, together with the shape of the two curves throughout the evolutionary process, indicates that the simpler characters are much more efficient in the evolution of concepts than the complex ones. There appears to be no advantage in the simple-to-complex order merely as such.

2. Concepts given to the subject directly, the common element concerned never being experienced in its concrete setting, shows about the same functional efficiency as concepts evolved from the concrete. The ability to define however is about twice as great with the former method. The power to define is thus in some cases at least a very inadequate index of the functional value of a concept.

3. A combination of abstract presentation and concrete examples yields a distinctly greater functional efficiency than either method alone.

4. During the evolution of concepts, mildly attracting attention to the common element *in situ* considerably increases the efficiency of the process. The increase is continuous from the

beginning of the process as far as it was investigated. But when the transition is made from the artificial evolution-material to that of the normal test-material the advantage is perceptibly decreased.

5. Moderate familiarity with each member of an evolution series comprising a number of concrete cases is perceptibly more efficient than twice as thorough familiarity with half as many cases. This advantage appears at the very beginning and persists at least through the first third of the process, i.e. as far as investigated.

6. Constitutional inferiors, dementia precox subjects and peretics average only about one sixth as rapid progress in evolving functional concepts as normals. The ability to define the concepts was very much poorer than that of the normals of the same functional level, it being almost totally lacking. The ability to form associations was greatly impaired, though associations once formed seem to be as permanent as with normals.

7. The curve of the evolution of concepts begins with an initial period of positive acceleration which goes on to a maximum rate after which a period of negative acceleration sets in which continues to the end of the process.

8. The individual concepts usually come into consciousness very gradually. Erroneous first impressions are either discarded or transmuted into the correct form by a continuous development. Trial and error plays, if not a dominating, at least a very great rôle in the process.

9. A considerable number of quantitative problems involved in the evolution of concepts may be conveniently investigated by slight modification of the general technique employed with the various experiments described in the present study.

BIBLIOGRAPHY

1. **Fisher**, S. C., The Process of Generalizing Abstraction; and Its Product, the General Concept. *Psychol. Rev. Mon. Supp.* 1916, 21. Pp. v+213.
2. **Grünbaum**, A. A., Uber die Abstraktion der Gleichheit. *Arch. f. d. ges. Psychol.* 1908, 12, 340-478.
3. **Hull**, C. L., The Formation and Retention of Associations Among the Insane. *Amer. J. of Psychol.* 1917, 28, 419-435.
4. **James**, **Wm.**, *Principles of Psychology.* 2 vols., New York, 1908.
5. **Ladd**, G. T., and **Woodworth**, R. S., *Elements of Physiological Psychology.* New York, 1911. Pp. xix+704.
6. **McMurry**, **Chas.** A., *The Elements of General Method,* New York, 1903. Pp. 331.
7. **Miller**, I. E., *The Psychology of Thinking.* New York, 1909. Pp. xxv+303.
8. **Moore**, T. V., The Process of Abstraction: An Experimental Study. *Univ. of California Pubs. in Psychol.* Vol. I, No. 2, 1910. Pp. 124.
9. **Pillsbury**, W. B., *The Psychology of Reasoning.* New York, 1910. Pp. viii+305.
10. **Preyer**, W., *The Mind of the Child,* Part II, translated by H. W. Brown. New York, 1901. Pp. xli+317.
11. **Queyrat**, **Frederic**, *L'abstraction.* Paris, 1895. Pp. 143.
12. **Ribot**, **Th.**, *The Evolution of General Ideas.* Translated by F. A. Welby, Chicago, 1899. Pp. xi+231.
13. **Ruger**, H. A., The Psychology of Efficiency. *Archiv. of Psychol.,* 1910, 2, No. 15. Pp. iv+88.
14. **Stout**, G. F., *The Groundwork of Psychology.* New York, 1903. Pp. vii+248.
15. **Thorndike**, E. L., *Educational Psychology.* 3 vols., New York, 1914.
16. **Thorndike**, E. L., *An Introduction to the Theory of Mental and Social Measurements.* New York, 1913. Pp. xi+277.
17. **Winch**, W. H., *Inductive versus Deductive Methods of Teaching,* Baltimore, 1913. Pp. 146.

GENERAL LIBRARY
JUN 28 1920
UNIV. OF MICH.

Vol. XXVIII
No. 2

PSYCHOLOGICAL REVIEW PUBLICATIONS

Whole No. 124
1920

Psychological Monographs

EDITED BY
JAMES ROWLAND ANGELL, University of Chicago
HOWARD C. WARREN, Princeton University (*Review*)
JOHN B. WATSON, Johns Hopkins University (*J. of Exp. Psychol.*)
SHEPHERD I. FRANZ, Govt. Hosp. for Insane (*Bulletin*) and
MADISON BENTLEY, University of Illinois (*Index*)

An Experimental Analysis of a Case of Trial and Error Learning in the Human Subject

BY

GEORGE S. SNODDY, Ph.D.
Professor of Psychology in the University of Utah, Salt Lake City

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.
Agents: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W. C.)
Paris (76, rue de Rennes)

Psychological Review Publications

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*J. of Exp. Psych.*)
JAMES R. ANGELL, UNIVERSITY OF CHICAGO (*Monographs*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*)
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews, notices of books and articles, psychological news and notes, university notices, and announcements, appears monthly, the annual volume comprising about 480 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 480 pages. (Suspended temporarily.)

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in May, and may be subscribed for in connection with the periodicals above, or purchased separately.

ANNUAL SUBSCRIPTION RATES

Review and Bulletin: $6. (foreign, $6.50). Journal: $3.25 (foreign, $3.50).
Review and Journal: $6. (foreign, $6.50). Bulletin: $3.25 (foreign, $3.50).
Journal and Bulletin: $6. (foreign, $6.50).
Review, Bulletin and Journal: $9. (foreign, $9.75).
Index with any other two: $7. (foreign, $7.50).
Review, Bulletin, Journal and Index: $10. (foreign, $10.75).

Current Numbers: Review, 65c; Bulletin, 35c; Journal, 65c; Index, $1.25.

THE PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. THE MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages, with a uniform subscription price of $5.50. (Postal Union, $5.80.)

Philosophical Monographs: a series of treatises more philosophical in character.
Library of Genetic Science and Philosophy: a series of bound volumes.

Subscriptions, orders, and business communications may be sent direct to the

PSYCHOLOGICAL REVIEW COMPANY

PRINCETON, NEW JERSEY

FOREIGN AGENTS: G. E. Stechert & Co., London (2 Star Yard, Cary St., W. C.);
Paris (16, rue de Condé)

I. INTRODUCTION

This study aims to make a detailed and exhaustive analysis of a simple case of trial and error learning. The tracing of a six-pointed star, as reflected in a mirror, was selected as the task to be learned—for the threefold reason that this task promised to involve a certain degree of difficulty for the learner; that improvement in learning to trace this figure promised to be rapid and to be open to accurate objective measurement; and that the time required to make a single complete tracing of the figure promised to be sufficiently short (the average time proved to be less than thirty seconds), to enable the learner to furnish a subsequent introspective description of his mental processes and procedure in complete detail.

Our method of investigation involved objective records and introspective analyses; and we aimed to obtain, from both these sources, a complete description of every step in the act of learning; it was also hoped that each method would furnish a check upon the accuracy of the other method.

It has been our endeavor to submit every stage of our act of learning to detailed analysis; and then to recombine the results of these analyses and to present them in their proper sequence. By this means it was hoped to obtain a complete genetic record of the act of learning at each stage of its development.

Psychologists have attempted in recent years to determine what differences obtain between learning in animals and in the human subject. The term 'trial and error' learning has come to be employed in certain quarters to describe the learning of the animal; and in certain quarters this term has been employed to designate a certain sort of human learning.

It seems probable that the term was first employed by students of animal behavior, and that its application to human learning is of a more recent date. The earliest use of this term which we have been able to discover appears in Galton's *Inquiries,*[1] where, however, one is led to suppose that the phrase was current at that time. It was soon pointed out that 'trial and success'[2] would describe this sort of learning more accurately than 'trial and error'; and in describing the learning of certain animals, Thorndike (46)[3] emphasized the significance of the 'accidental success' which attended certain reactions of his animals when placed in the learning situation. He also points out that human learning differs from animal learning in that the latter is characterized by an absence of independent ideas and by a lack of continuity in the mental stream. In a later study[4] he differentiates three sorts of learning: "Learning by trial and accidental success,—that is, by the strengthening of the connections between the sense impressions representing the situation and . . . our successful response to it, and by the inhibition of similar connections with unsuccessful responses"; learning by means of imitation; and learning by means of ideas.

---

[1] Francis Galton, Inquiries into human faculty, London, 1883, p. 171. In describing the movement of the eye over the details of the 'fire face' in the burning coals, Galton speaks of it as "being guided partly by the easy sequence

Lloyd Morgan (26) employed the term 'trial and error' learning to refer to that type of behavior which obtains in the dog when he learns to meet a situation successfully. Morgan believes that the dog's reactions to a situation are immediate responses which owe their origin either to the animal's racial or his individual experience; the behavior of man, he points out, is of a wholly different sort, in that man approaches a problem deliberately, marshals his past experience, and 'reasons out the difficulty' before he acts.

In more recent years experimental investigation has shown that a fundamental distinction between human and animal learning can be drawn only with the greatest difficulty. In investigations of human learning Ruger (37) and Book (4) find that most of the serviceable procedures or principles which arise in

of muscular movements, partly by idiosyncracy, partly by the mood and the associations present at the time, until finally a picture that will 'suit' is built up in this trial and error fashion."

[2] Ladd and Woodworth, Elements of Psysiological Psychology, 1911, p. 550, in referring to the 'trial and error' method of learning, say,—"We prefer to call it learning by varied reaction through selection of the successful variants."

[3] Figures refer to Bibliography at end of article.

[4] E. L. Thorndike, The mental life of the monkeys, *Psychol. Monog.*, III, 5, 1901, p. 2.

the course of the learning, are hit upon by accident. This finding is leading psychologists to question the existence of a specifically human type of learning to meet situations.

The term 'trial and error,' however, is still current in the sense in which Jennings (19) employs it to describe the behavior of Paramecium. Jennings points out that Paramecium's reaction to an obstacle in its path is of a single sort,—the avoiding reaction. If this reaction does not bring immediate success, it is repeated again and again until the animal ultimately succeeds in avoiding the obstacle; Baldwin has called this the 'try, try again' method. It is in this sense that the term trial and error is employed in the present study of mirror tracing. However, as we shall note later, the human subject employs many sorts of reaction in any given situation, instead of having recourse to but one reaction, as in the case of Paramecium.

The details of trial and error learning have never been investigated in any thoroughgoing fashion, except in the domain of animal behavior; and it would carry us too far afield to refer to all of these animal studies here. However, in later sections of this paper, we hope to touch upon certain problems which these studies have raised.

Investigations by Perrin (34), by Hicks and Carr (16), and by Starch (42) have discussed certain phases of trial and error learning in man. Hicks and Carr investigated the procedure of children and of white rats in learning to find their way through a maze; Perrin studied the adult's procedure in solving a similar problem. The act of learning investigated by Starch consisted in tracing a figure by mirror reflection.

In the opinion of the present writer, none of these studies have attempted to bring the trial and error phase of human learning into relation with later developments in the process of learning. They have usually assumed that learning begins with a process of trial and error and ends in a 'controlled' and 'purposeful' procedure, and they have made no serious attempt to supply the intermediate links between these terminal points. Most of the discussions of the topic of learning which one finds in the textbooks are open to the same criticism; Watson (53) is a notable exception to this criticism.

II. EXPERIMENTAL PROCEDURE

I. Apparatus

Several forms of apparatus were employed in this investigation; all of these, however, were variants of the six-pointed star, with certain accessory mechanisms.

Apparatus I. This ~~initial form~~ of apparatus consisted of a 'star' design and a recording mechanism. The former was constructed as follows: A regular and equilateral star, whose extreme width was 17 cm., was cut from a sheet of brass one mm. thick, and was mounted upon a glass plate six mm. in thickness. By this means we obtained a path, in the form of a six-pointed star, which was six mm. in width, whose edges were of brass, and whose base was of glass. (Fig. No. 1.) The task assigned to the learner consisted in learning to trace this path, as seen by reflection in a mirror, without touching the brass border. Earlier experiments had revealed a tendency on the part of certain learners to slide the metal stylus along the edge of the path, keeping the former in contact with the latter throughout; in order to eliminate this procedure, niches were cut at intervals along the brass border. There were four of these niches on each of the twelve sides of the six-pointed figure,—the niches being three mm. wide and three mm. deep, so that they were large enough to admit the point of a stylus. The stylus was made of brass with an ivory tip, and was of the size and form of an ordinary lead pencil. The two brass plates, whose edges constituted the edges of the path, were connected with a binding-post at the edge of the glass plate (*A*, Fig. 1). A metallic brush-contact was inserted into a second binding-post in such fashion that it extended out over the path at the starting point, (*B*). A flexible copper cord connected the upper end of the stylus with a third binding post, (*C*).

The recording mechanism consisted of a kymograph which carried an endless sheet of white paper. Three fountain pens, which were attached to three electro-magnets, were in contact

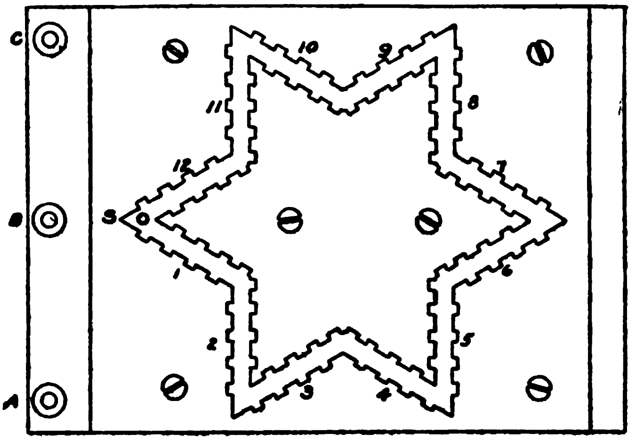


FIG. 1. This is a drawing showing the "star" apparatus employed in this investigation. The starting point for tracing the diagram is the small circle to the right of the point *S*. See text for details of the construction of the apparatus.

with this band of paper. One of these electro-magnets was in circuit with the two brass plates which constituted the margins of the outline of the star; this electro-magnet, with its pen, recorded any contact which might be made between the stylus and the edge of the path. The second electro-magnet was in circuit with the brush contact in the path of the star; and indicated the beginning and end of a circuit. The third electro-magnet was in circuit with a Jacquet chronometer, and by this means a time line, marking fifths of a second was recorded, (Fig. 19, p. 61).

The star was placed upon a flat-topped laboratory table, which was of convenient height for writing. A mirror 18 cm. by 20 cm. was fixed on the table in a vertical position, with its reflecting surface turned toward the star. A screen of green blotting paper was fastened in such a position that the subject could readily and conveniently see the mirrored image of the star, but he could not see the star itself. The apparatus was adjustable so that a natural and convenient posture of the learner was secured throughout the experiment.

Apparatus II. This second form of apparatus was designed for the two-fold purpose of furnishing an accurate and detailed graphic record of every movement and change of direction made by the stylus in tracing the outline of the star, and of recording not only the time of each movement and change of direction, but also the total time of the complete circuit. This apparatus consisted of a 'tapping recorder' and duplicate copies of a six-pointed star. The tapping recorder was constructed as follows: A thin plate of brass, thirty-five cm. long, thirty cm. wide, and 1 mm. thick, was mounted upon a wooden frame, which rested upon four wooden supports. This formed a box-like structure, of which the brass plate constituted the top. Pieces of felt were inserted between the wooden frame and the brass plate, and the latter was loosely attached to the frame by means of screws. A large electro-magnet, which actuated a heavy hammer, was placed below the brass plate in such a position that the stroke of the hammer was received at the center of the plate. The sheet of paper which bore the diagram of the star was clamped securely to the upper surface of the brass plate; a second sheet, bearing a duplicate diagram of the star, in perfect register, was introduced between the first sheet and the brass plate, and a sheet of carbon paper was inserted between these two diagrams.

A mirror and a screen were arranged in positions similar to those described in our initial form of apparatus. The electro-magnet was in circuit with a battery of dry cells and a Jacquet chronometer in such fashion that each beat of the chronometer was attended by a hammer stroke upon the under side of the brass plate. Each stroke of the hammer caused a slight movement of the plate and recorded a dot on the line traced by the stylus; hence the record shows the detailed movements of the stylus, together with the time of each movement. (A sample record is shown in Figure 9, p. 41).

Before beginning this group of experiments the learner was given considerable practice with this mechanism, during which details of various sorts were traced in free-hand fashion in direct vision—this being done in order to make sure that the tapping did not interfere with the free movement of the stylus, and in order to familiarize the subject with the apparatus. It may be added that no such interference was detectable; signatures which our subjects wrote under these experimental conditions could not be distinguished from their usual signatures, excepting that the time-dots were present in the former case.

Apparatus III. This form of apparatus was a slight modification of that employed by Freeman (14)[5] in his investigation of handwriting. The essential features of this apparatus were as follows: A continuous band of adding machine paper is drawn across a flat writing surface by means of an electric motor; over this moving band is laid a stationary sheet upon which the observer writes with an ordinary lead pencil. The reader is referred to Freeman's two papers (op. cit.) for an elaborate and accurate discussion of the technique involved in the measuring of the time of hand movements by means of this apparatus.

In our modification of this apparatus we employed a plate fibre-board from which two sides of the star (sides 3 and 4, Fig. 1) were cut out in the same way as these two sides were cut out of brass in our metal star,—the two grooves thus furnishing paths whose directions were at right angles to each other. This fibre plate was fixed to the writing surface in such fashion that when the subject sat before the apparatus, the path to be traced lay directly before him in the form of an inverted V; the moving band of paper passed under this V, moving from right to left. After each circuit of the diagram had been completed, the primary record and the secondary record (upon the moving band) were filed away and preserved together. A sample of one of these primary records is shown in Figure 12, p. 49).

This apparatus was used in making a detailed investigation of the learning in the case of two sides of the star which proved to be very difficult; it enabled us to determine the time of each movement, the time of each pause, and the location of each pause. In this group of experiments we employed unpracticed learners who had never had any previous experience in mirror-tracing; but we also employed this apparatus with certain of our practiced learners in order to make a more detailed investigation of certain features of their behavior while tracing at high speed.

---

[5] Also, An experimental analysis of the writing movement, Psych. Rev. Mono. Vol. XVII, No. 4, will be very helpful to the reader in understanding this apparatus.

Apparatus IV.[6] The object of this fifth group of experiments was to obtain an accurate photographic record, with time determinations, of all the movements made by the observer in tracing the path of two of the more difficult sides of the figure. The method was as follows: The path to be traced was drawn upon a sheet of coordinate paper. Tracing was made by means of an ordinary adjustable pencil which carried (just above the writing-point) a small rice-grain electric bulb, and this bulb was in circuit with a tuning-fork (twenty-five vibrations per second). The bulb was shielded by a small cup of hard rubber. The diagram to be traced was mounted on a flat table in a position convenient for writing; above the diagram was a frame which carried a camera, in such a position that the lens of the camera was directly over the diagram.

Before the observer began his tracing the photographic plate in the camera was exposed to the diagram; then the lens was closed for a time while the tuning-fork was being set in vibration, thus causing a series of flashes (twenty-five per second), in the lamp at the pencil-point. The lens was opened again when the learner began to trace the figure with his pencil. The photographic record of this tracing shows the path followed by the learner in making the tracing, and also the time measurements, made in one-twentyfifths of a second. A sample record from this machine is shown in Figure 10, p. 47.

This apparatus was employed chiefly for the purpose of comparing the tracing of a slightly practiced learner with the tracing made by the same observer after he had reached high speed,—the prime object of this apparatus being to furnish a more accurate and detailed record of the temporal relations.

2. METHOD

All of our experiments were performed in the Psychological Laboratory of Clark University; excepting a few experiments conducted at the University of Utah for objective data only. All of our data will be referred to the type of apparatus employed and the condition of the subject (practiced or unpracticed, beginner or advanced learner). We sought to obtain data from objective and introspective sources at every stage of the learning. In our introspections the observer[7] first reported his description in detail; this was then followed, where necessary, by careful questions which were designed to clear up obscure points, and to give the experimenter a clearer understanding of the procedure and processes involved. Where questions were employed, their exact wording was recorded and will be noted in our quotations.

---

[6] We are indebted to Mr. Frank G. Gilbreth, of Providence, R. I., not only for the loan of valuable apparatus employed in this group of experiments, but also for numerous valuable suggestions.

Every effort will be made to keep conditions uniform throughout the practice of each reagent. The apparatus was explained in detail before the experiment began, by which means we hoped to secure a constant and uniform behavior from each learner; since any curiosity on the part of the learner,[8] regarding the use of the apparatus, was satisfied at the outset and his attention was left free for the experiment. We were frequently amazed by the constancy of behavior of the learners, and at the almost complete absence of irrelevant and disturbing elements.

Since all of our apparatus involved the tracing of a diagram by means of a stylus or pencil, care was taken throughout that the height of the tracing surface, and the distance of the hand from the body, should be similar to these requirements in the ordinary act of writing. The learner sat in a comfortable, cushioned chair; and efforts were made to secure perfect quiet in the experimenting room. In those cases where a slight noise arose from the operation of the apparatus, our learners assured us that this did not constitute a distracting factor; and this statement is abundantly verified by our introspections.

In an early stage of our preliminary investigations, it was noted that the instructions and the distribution of practice were important factors in the learning. Accordingly, we decided to vary both the instructions and the practice periods in such fashion as to furnish positive data regarding the influence of these two factors. In the descriptions and discussions which follow, we shall make frequent use of the terms 'recess period,' 'recess period practice' and 'series practice.' By recess period we mean that period of time which the experimenter interpolates between successive efforts of the learner. Each such effort is a unit of practice; it may involve a tracing of one side of the star, or a tracing of the whole star. The term 'recess period practice' refers to that practice in which a recess period appears before each unit of practice, whether this latter be a tracing of the third side of the star or a circuit of the whole star. The term 'series practice' refers to that practice where the unit is repeated one or more times without the intervention of a measurable recess period,—for instance, five circuits of the star made in immediate succession without any intervening recess period, would constitute a series practice.

---

[7] Throughout this paper the learner who also furnishes an introspection of his learning will be arbitrarily designated an 'observer' while the learner who furnishes merely objective data will be referred to as 'subject'; under certain conditions a learner may act in both capacities.

[8] A special instance of this was the question,—'Will I get an electric shock from the apparatus?'

Three types of instruction were employed in this investigation. In the first type the instructions were as follows: "Attend to both speed and accuracy in going around the star; that is, avoid touching the sides of the path, and go around as fast as possible," (speed and accuracy instructions). Care was taken to repeat these instructions at least twice to the learner.

Since the two factors, speed and accuracy, might differ in value according to the order of their mention in the instructions, the order was reversed in the second type of instruction, where it was as follows: "Go around the star and avoid touching the sides; your efficiency will be measured solely in terms of your accuracy," (accuracy instructions). In the third type of instructions, the learner was instructed as follows: "Go around the star as fast as you can; your efficiency will be measured solely in terms of your speed," (speed instructions).

We were also interested in testing out some of Langfeld's (23) conclusions on the effect of instructions; accordingly, we modified our instructions so that the subject was instructed as follows: "You will trace around the star, keeping the stylus in the middle of the path."

After one group of conditions had been investigated we changed the instructions in order to investigate the effect of such change. Throughout our discussion the data will be referred, in every instance, to the instructions under which these data were obtained.

Various distributions of practice were investigated; each learner, however, was kept under constant conditions until the effect of those conditions had been investigated. It was our aim to obtain data regarding both recess period practice and series practice at every stage of the learning, and to correlate these two types of practice with the type of instructions. In other words, we were concerned, not with the economy of learning, but with the effect of the instructions; it was our aim to discover the correlation between distribution of practice periods and the attainment of a high degree of accuracy or of speed under specific instructions.

3. LEARNERS

In our investigation we employed twenty-one learners; nine of these were used for securing objective data and introspection, and twelve were used for objective data only; the former are designated 'observers,' and the latter, 'subjects.' Our observers were all graduate students in the department of Experimental Psychology in Clark University; the first two in the following list of observers having attained the doctorate and the remaining seven, the master's degree in psychology. Our subjects were advanced students in Clark College, Clark University, and the University of Utah. The learners will be referred to throughout this paper by number only. A college class of advanced students from the University of Utah was also used in two group tests.

Observers: (1) S. W. Fernberger, (2) Sara C. Fisher, (3) H. R. Crosland, (4) B. W. DeBusk, (5) Florence Mateer, (6) F. J. O'Brien, (7) Bruce B. Robinson, (8) R. B. Teachout, (9) R. H. Wheeler.

Subjects, for objective data only, were:
(10) Ethel Bowman, (11) G. A. Coe, (12) Ruth Dunlop, (13) Florence Groshell, (14) Chinaki Kageyama, (15) Eva Kass, (16) M. C. Knapp, (17) Yashihide Kubo, (18) H. C. Lewis, (19) D. I. Pope, (20) Loren Richards, (21) Rex W. Williams.

III. RESULTS

I. THE LEARNING CURVE: CORRELATION BETWEEN THE EFFECT OF THE INSTRUCTIONS AND THE DISTRIBUTION OF PRACTICE.

A. General Character of the Learning Curve

As the reader will observe by referring to Figures 5, 6 and 7, the nature of the curve for this type of learning is dependent both upon the instructions given to the subject and upon the distribution of practice. When a mixed practice (recess period followed by series practice) is given, the results of which are shown in Figs. 5 and 7, the general trend of the average curve—the arithmetical average of the time and error quantities—is similar to that obtained by using animals in a maze.[9]

The curve may very readily be divided into two distinct parts: First there is an initial drop where great improvement takes place and where, in most instances, the time and error curves are parallel; and second, a plateau stage, where there is much less improvement and where the time and error curves may often, as in Figs. 4, 6 and 7, tend to vary in opposite directions. If a curve representing the arithmetical average of the time and error quantities should be drawn between the time and error curves in Fig. 7, a curve of logarithmic character is obtained which is roughly similar to that obtained by Ebbinghaus in his investigation of forgetting. In our analysis of the learning curve we shall secure better results, however, if we take up a consideration of the type of curve secured under every possible combination of instructions and distribution of practice; which we shall proceed to do in the following sections.

B. The Learning Curve under Accuracy Instructions

a. *Series Practice from the Beginning of the Learning.*—By referring to Fig. 2 the reader will observe that no improvement

---

[9] The reader is referred to Thorndike's Animal Intelligence, Macmillan, New York, 1911, and to Watson, Behavior, Henry Holt, New York, 1914, for numerous examples of such curves.

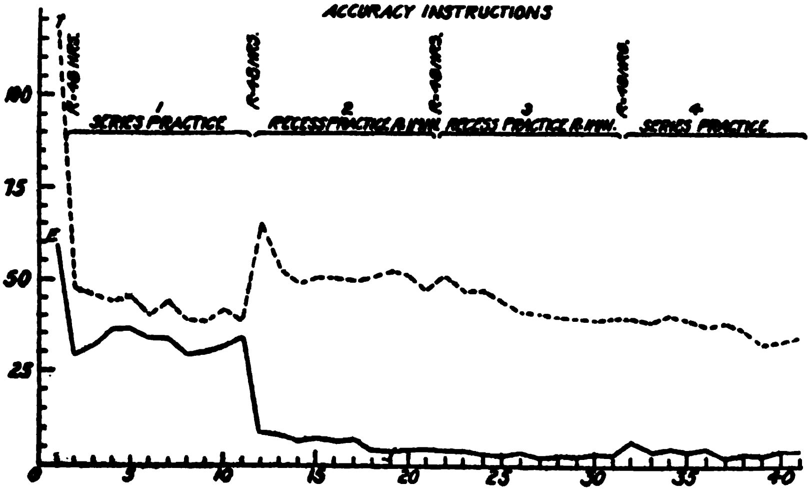


FIG. 2. A diagram showing the average learning curve of ten subjects working under various conditions of practice. From the scale at the left may be read the time in seconds and the number of errors. The horizontal scale indicates the successive circuits of the star. It should be noted that the improvement in accuracy (errors) is confined to the recess period practice. The rise of the time curve in Part II is an *irradiation* curve. The quantitative data for each subject are shown in Table II, p. 79.

took place in accuracy while these subjects were working under series practice conditions. The instructions stressed the avoidance of errors and no limitations were placed upon the time required to make the circuit of the star; the subjects were striving merely to avoid errors. This same state of affairs obtains with all our subjects working under the same conditions. This experiment has been performed with thirty objects during the course of our work and we have never succeeded in getting appreciable improvement under such conditions. No improvement at all occurred except in the case of three subjects who show slight improvement. This is true only when the subjects have had no experience with mirror tracing, which is essential for this case, as later results will show that this state of affairs holds only for the initial drop of the learning curve, (Fig. 21, p. 65).

b. *Recess Period Practice throughout the Learning.*—Under this form of practice the accuracy instructions were used and the circuits of the star were separated by recess periods. Reference

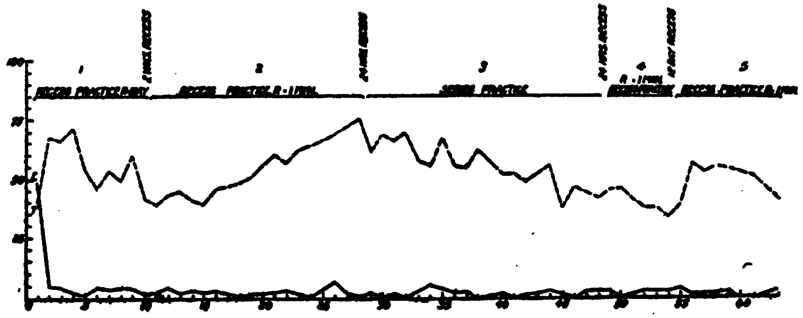


FIG. 3. Ordinate indicates time and errors; abscissa, successive circuits. This learning curve was secured from the work of subject No. 13; accuracy instructions were employed throughout. Part II of the practice shows a very marked irradiation curve. It should be noted that series practice in Part III brings about a marked improvement in speed without resulting loss in accuracy. The curve here shown is taken from data secured after the subject had completed a recess and a series practice, such as is shown in Part I of Fig. 2.

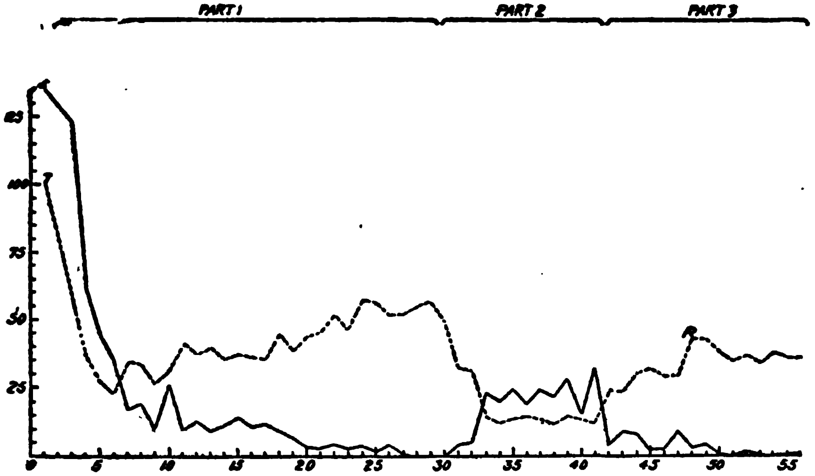


FIG. 4. Ordinate indicates time and errors; abscissa, successive circuits upon apparatus I. This is a curve of Subject No. 14, secured through a practice of one circuit of the star per day. Part I was secured under accuracy instructions, Part II speed and accuracy, Part III under accuracy instructions. The point *R* indicates a recess period of eight months. It should be noted that, by the ninth circuit, the subject had reached the limit of his improvement, under recess practice conditions, as measured by the arithmetical average of the time and error quantities; thereafter, he makes a choice of one element and neglects the other.

to Fig. 4, will show that, under recess period practice, the improvement in both accuracy and speed in the early stages of the learning, is very rapid; but this is followed by a change of direction of the time curve. This marked rise of the time curve is explained in our objective and physiological analysis of the learning as irradiation; which has led to our naming this type of curve, an *irradiation curve,* (page 51).

The time curve is seen to rise progressively for a number of circuit; the increase in time is paralleled by a fall (improvement) in the error curve. This type of curve is found under no other conditions than recess period practice with accuracy instructions. The rise in the time curve is followed by a fall, provided the practice is continued sufficiently long, Figs. 2 and 3. It should be

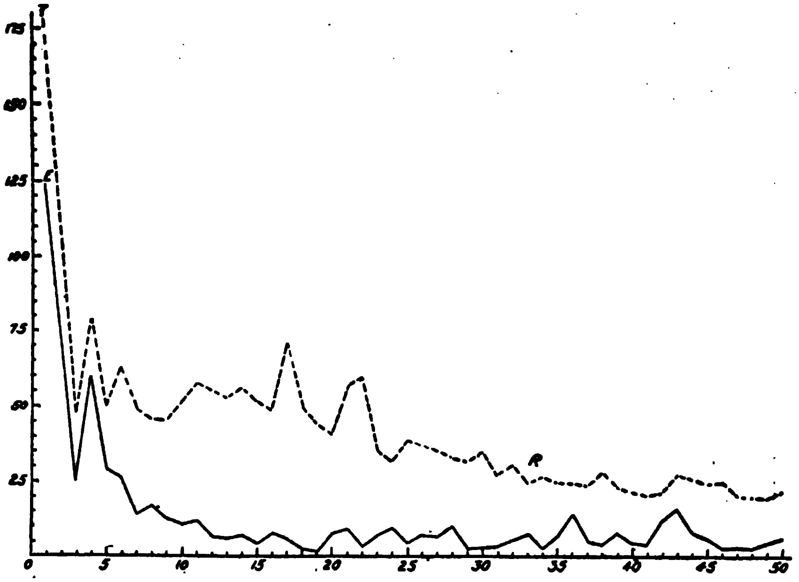


FIG. 5. Ordinate indicates time and errors; abscissa, successive circuits upon apparatus I. This is the curve of Subject No. 17, secured under accuracy instructions. One circuit per day was the scheme for the first two circuits; thereafter, two circuits in series per day was the practice. It should be noted that there is a simultaneous fall of both time and error curves throughout the learning,—a marked contrast to state of affairs in Fig. 4 where recess period practice is maintained throughout. The fourth circuit, the first not to be preceded by a recess period, shows a loss in both time and errors; this would not have occurred had the initial drop in the learning curve been completed before the series practice was begun. The point *R* indicates a recess interval of eight months.

noted that there is no reversal in the error curve, which, on the contrary, may show improvement or remain unchanged so long as constant conditions are maintained.

c. *Recess Period Practice at the Beginning, followed by Series Practice.*—The effect of this set of conditions is clearly shown in Fig. 5. In Figs. 2 and 3, it is seen that, at certain later stages in the learning, series practice is of positive advantage; the negative improvement in the time curve is reversed, while there may be no loss in the error curve.

C. The Learning Curve under Speed and Accuracy Instructions

a. *Series Practice from the Beginning of the Learning.*—Under these conditions the quantitative results are identical with those secured under series practice with accuracy instructions, as noted above. Our introspective results (see a later section of this paper) show, when the element of speed is included in the instructions to beginners, that, after the first few strokes, the instructions

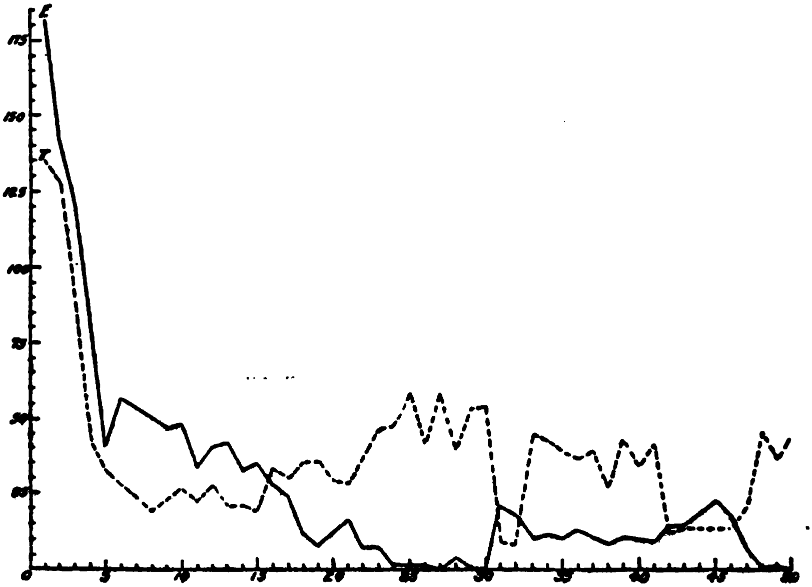


FIG. 6. Ordinate indicates time and errors; abscissa, successive circuits. Curve of Subject No. 16, secured under speed and accuracy instructions, with recess period practice of one circuit per day. Note that both the time and error elements alternate as in Fig. 4, thus indicating that the basis of the alternation is the recess period practice and not the instructions to the subject.

are "discounted" and the subject proceeds to trace the star with an 'accuracy only' *Aufgabe.* No improvement at all can take place in the early stage of the learning until a recess period is inserted between circuits,—a case we have already discussed.

b. *Recess Period Practice from the Beginning of the Learning.*—Fig. 6 presents a learning curve secured under these conditions. In this curve it will be observed that the early high rate of improvement is shown in both the time and error curves. Throughout the remainder of the curve there appears to be an alternation in the improvement of the time and errors, i.e., a selection of one or the other element of the learning. In other words, it seems that the attention of the subject may be directed to an elimination of errors or to the securing of a high rate of speed, but may not be directed to both simultaneously; at any rate a high degree of efficiency cannot be secured in both elements of the learning at the same time, excepting in the very earliest stage of the learning, which we have called the initial drop of the curve. In later sections we hope to show why simultaneous improvement cannot occur in the time and error curves under recess period practice conditions.

c. *Recess Period Practice at the Beginning of the Learning, Followed by Series Practice.*—Under these conditions we secure a learning curve (Figs. 5 and 7) which shows improvement in both time and errors while the recess period conditions are being maintained during the initial drop of the learning curve; and a persisting improvement occurs in speed throughout the mixed recess and series practice. This type of time curve has never been secured under any other conditions, which may be taken to show the function the series practice performs in securing improvement in speed without loss in accuracy. It should be noted that the practice conditions here are mixed, i.e., recess period practice for the first stage of the learning, and recess and series practice during the remainder of the experiment. This type of practice has uniformly given us the highest net efficiency,[10] the most uni-

---

[10] Throughout this study we have avoided the use of any mathematical formula for computing efficiency. Such a formula would necessarily involve both the time and error quantities and consequently would lead to definite

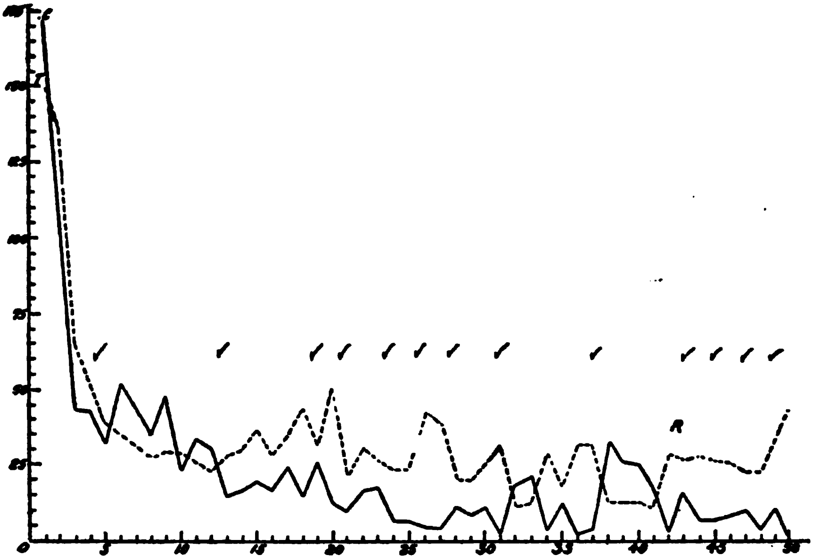


FIG. 7. Ordinate indicates time and errors; abscissa, successive circuits. Learning curve of Subject No. 6, secured through recess period practice, one circuit per day, for the first four days, and mixed series and recess period practice thereafter. The check (V) marks indicate recess periods of at least one day; circuits not so indicated were in series. Note the synchronous fall of both time and error curves up to the thirtieth circuit. The point *R* indicates a recess period of nine months.

form learning curves, and is precisely that type of practice which is usually found in the uncontrolled learning of practical life; excepting that, in the latter, the recess period is usually not purposely inserted in the early stages of the learning.

D. Discussion

From the foregoing presentation of learning curves the reader will observe that the two important objective factors in the learning are the recess period and the instructions to the subject. We have seen that the recess period conditions the improvement in

loss, since, in that case, we should lose sight of the changes taking place in each element of the learning under the different conditions of practice; and it is just such changes which seem to be clearly shown by our simple learning curves, indicating both the time and error quantities. Whenever a general unanalytical term, showing improvement in a rough way, was desired, we have employed the arithmetical average of the time and error quantities.

accuracy in the first stage of the learning; while, in later stages of the learning, improvement in speed, without a resulting loss in accuracy, is dependent wholly upon series practice or a long repetition of recess period practice.

Later sections of this paper will show that there are two types of improvement in speed; one occurs during the initial drop of the curve and is due to a shortening of the path taken by the stylus in tracing the star, and to the elimination of delays when the stylus is caught in the niches,—this improvement in speed results indirectly from the gain in accuracy; and, there is an improvement in speed of another type, which results from a physiological change—facilitation—in the neuromuscular apparatus, brought about by series practice or a long repetition of recess period practice.

The conclusions which we have reached in this preliminary survey of our data may be summarized as follows:

(1) Without the recess period practice under accuracy instructions, the initial drop in the learning curve,—the place of greatest improvement,—cannot be secured; in fact, no permanent improvement in accuracy may be secured in the early part of the learning, without the interpolation of a recess interval between the circuits of the star, Fig. 2.

(2) If the recess period practice, under accuracy instructions, is continued beyond the initial drop in the learning curve, the time curve, will show a change from positive to negative improvement, —our irradiation curve,—and thus continue to show progressive loss, until, through repetition, it begins to show positive improvement; while the error curve continues to show improvement, or remain stationary, throughout the practice, Figs. 3 and 4.

(3) After the initial drop in the learning curve is completed, mixed series and recess period practice will lead to a simultaneous improvement in speed and accuracy, Figs. 5 and 7. Recess period practice under the same conditions leads to improvement in but one element selected by the learner, either speed or accuracy, and to a loss in the element not selected,—thus, when accuracy is chosen the time curve becomes our irradiation curve, Figs. 4 and 6.

(4) Recess period practice leads to deliberate, slow movements, favoring accuracy; series practice leads to rapid movements and favors speed, Figs. 2 and 3.

(5) In general summary, we may say that in the first stage of the learning, the recess period is the important factor, and emphasis upon accuracy is encouraged. In later stages of the learning, series practice becomes necessary, which secures simultaneous improvement in speed and accuracy, making the speed and accuracy instructions appropriate.

2. INTROSPECTIVE ANALYSIS OF THE LEARNING

By far the greater part of our introspections are drawn from work with our first form of apparatus. This apparatus was chosen largely because the time of making a circuit of the star was short enough to admit of detailed introspective analysis, and also because the geometrical form of the star,—its being divided into twelve equal parts, greatly aids the learner in recalling the details of his experience. Another factor which facilitated introspection was due to a second mechanical feature of the star. By referring to Figure 1, it will be seen that the star is so constructed that, when the learner is seated directly in front of the apparatus, four of the sides of the figure run in a direction which is parallel to the median plane of the body and eight sides run obliquely to this plane; the former we have called 'vertical' and the latter 'oblique.' The mirror reverses the direction of the oblique lines so that the mirror image runs nearly at right angles to the true direction of the groove; the four grooves which are parallel to the median plane of the body are changed in the mirror image through one hundred and eighty degrees and, in tracing the star, one moves in a direction which is exactly opposite to the apparent direction. The learner soon discovers this reversal, and from the second circuit of the star he finds that these four sides are very easy to trace.

The star is thus divided, for the learner, into difficult and easy sides after the first circuit of practice. This differentiation was found to be of the highest value in securing an analysis of the learning difficulties; since the difficult directions are contrasted with the easy directions in the learning situation. The oblique sides vary in difficulty from time to time with each observer; but they are always the most difficult parts of the star for every observer. Experiences which have to do with these oblique or difficult sides stand out prominently in our introspections.

Our introspections reveal the fact that in every respect the learning of mirror tracing is of the true trial and error sort. In a few instances, in the earlier stages of the learning, subjects would attempt to 'reason out' the true direction of the grooves; but in all cases, in our experiment, this was immediately abandoned as of no value, and the subjects fell back into the stage of trial-and-success striving, until, by the accumulation of successful movements, they finally succeeded in making a circuit of the star. The first successful movements are made accidentally, so far as the conscious experience of the learner is concerned.

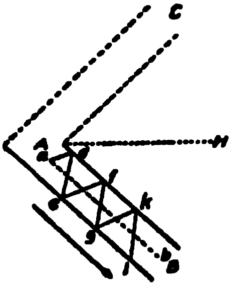


FIG. 8. A diagrammatic tracing of the first side of the star. The zigzag path *ad, de, ef, fg* is an early tracing of the path *AB*, while *ab* is the first tracing after a recess interval; with series practice the zigzag path remains. The diagrammatic distortion lies in the fact that a few subjects will not have the zigzag path reduced to a smooth one so quickly. Figs. 2-7, inclusive, show these facts in quantitative form.

A. Making the Start

In beginning his tracing of the first side of the star, the learner employs a movement which would be appropriate for tracing this side if the apparent direction were the true direction of the path; that is, his movement is practically at right angles to the true direction of the path. This movement leads to the stylus being caught in a niche on the right hand side of the path, *d* Fig. 8. The subject endeavors to withdraw the stylus but each effort serves to force it more tightly into the niche; the attempts to withdraw are repeated again and again, the subject endeavoring, subjectively, to withdraw, while, in reality, he is forcing the stylus more tightly into the groove.[11] Our objective results show that some subjects remain in the first niche as long as seventy seconds; this, however, is exceptional.

During the period when the stylus is caught in the niche a gradually increasing strain develops in the muscles of the fingers, wrist and arm, and finally includes the muscles of the back. There is an increasing tension in the muscles of respiration which is accompanied by at first a partial, and at times, a total inhibition of breathing. The whole experience is unpleasant and in some cases is described as painful, especially after the experience has persisted for some time; this is terminated by a relaxation, and in some cases a 'giving up,' the degree of this relaxation varying with the observer. In most cases there is complete relaxation in the tensions of the arm and body; the observer often falls back in the chair, ~~but~~ this does not occur in all cases. The most characteristic physiological change is that which takes place in the respiratory apparatus; this consists in a relaxation and then a complete reinstatement of breathing.

The state of general relaxation usually continues for a short time; it is terminated by the beginning of imaginal processes which are directed to discovering the true direction of the groove. At this time there are often visual images of the groove and kinaesthetic manual-motor images of making movements down the groove. These imaginal processes are immediately cut short by a return of the eyes to the objective situation. Quite often the movement which had been made before and which constituted the attempt to go in the direction the mirror indicated, is repeated; and following this repetition there is often a complete reversal of the movement made.

This new movement (*de.* Fig. 8) is brought about by the operation of those muscles which are, physiologically, antagonistic to the muscles controlling the first movement made by the observer. It is our belief that this reversal is purely reflex and can be accounted for by the laws of physiological reversal,[12] since the observer introspects that he 'found his hand moving in the proper direction.' This movement carries the stylus to the niche at *e*, and here the state of stress and strain which occurred at *d* is re-experienced, except that it endures for a much shorter time: all of which is followed by a reversal of the last movement (*de*). This new movement is inhibited by pressing the stylus downward at *f* and swerving in the direction of *g*,—a process continued throughout all the difficult sides.

---

[11] In connection with this procedure of the subject an interesting thing occurs. He often reports to the experimenter that the sides of the star are magnetized and consequently are holding the stylus tip against the edge of the path; while, in reality, the subject is moving in a direction contrary to his subjective awareness.

While our description here is schematic, in the sense that the reactions of many subjects are, at first, much more complicated; yet, it is essentially true for all, and is duplicated precisely in some of the reactions of all of our subjects. Those whose reactions are more complicated at first will eventually reproduce a pattern as simple as the above. Throughout the execution of these movements, the observers introspect that they *accidentally* make the movements which lead them out of the difficult niches; the reversal of movement, when it does come, is so quick that it takes place wholly without conscious direction.

B. Preliminary Analysis of a Circuit

The first side is much the most difficult side during the first circuit of the star. Having traversed the first side, the observer has little difficulty with the second side,—the reader will observe (Fig. 1) that this is one of the sides running parallel to the median plane of the body. The path of the stylus on the second side is usually a straight line and is made at much higher speed than the first side. The second side is one of the subjectively 'easy' sides; and the tracing here is always accompanied by a marked feeling of pleasantness; the observers invariably introspect that this side is made with rather long, sweeping movements. When the turn toward the third side is reached, the observer meets with another difficulty; this is another oblique side which runs at an oblique angle to the median plane of the body.

---

[12] Our section on the objective analysis of the learning situation will discuss at some length the physiological points here involved.

The zigzag movements, made on the first side, are instituted again on the third side; the progress up the side proceeds in exactly the same fashion, but is usually more rapid than on the first side. Here the introspections show that the successful movements, as in the case of the first side, are hit upon by accident. The tracing is characterized by unpleasantness which in some cases is described as more intense than on the first side; the time is usually not quite so long as in the tracing of the first side, but the errors are often much less. The zigzag strokes are in many cases inhibited by a downward pressure on the stylus which is responsible for the reduced number of errors. These inhibitions are reflex and come when the stylus is seen to approach the side. Many of the observers refer the unpleasantness which is experienced in tracing an oblique side to these inhibitions of the forward movement.

The fourth side is another of the oblique sides and consequently is one of the difficult sides where the zigzag course of the stylus is continued. The time of the tracing and the number of errors usually remain about the same as on the third side; and, when a subjective estimate of the difficulty is asked for, it is usually reported as difficult as the third side. The fifth side is one of the easy sides, and the path of the stylus here is straight and few errors are made in tracing it; and this side is quite often described as being a resting place where the observer relaxes a little after his efforts made on the third and fourth sides.

The sixth and seventh sides are found to be difficult; the zigzag course of the stylus appears again; the errors and the time remain about the same as on the third and fourth sides. The special difficulty connected with the seventh side consists in the fact that it extends to the left. On all of the oblique sides thus far, the stylus has moved toward the right; and the turning to the left, which now begins, is especially hard to acquire, certain learners reporting that this is by far the most difficult of the sides.

Having traced the seventh side, the remainder of the star seems to be comparatively easy. The oblique sides are relatively difficult, however; but they are usually reported to be much easier than the oblique sides in the first half of the star. The twelfth side varies in difficulty with the different subjects, yet it is almost always one of the first of the oblique sides to become easy. The imaginal material reported by the subject throughout the tracing is very meager, when working under accuracy instructions, and none of the observers has ever reported the presence of a control image during the first five circuits of the star.

After this preliminary survey of a circuit of the star, we take up our introspective analysis in detail, quoting freely from the introspections of our observers.[13] These introspections will be drawn from different stages of the learning because of the contribution which they seem to make to the analysis of such stages.

C. The Reflex Nature of the Learning

a. *Getting Out of a Difficulty.*—As noted above, the first movement the observer makes is in the wrong direction and goes in the apparent direction of the groove. The experiences here are characterized by an awareness of repeated efforts to withdraw the stylus; these are merely attempted movements which go in the wrong direction and serve to drive the tip of the stylus more tightly into the niche. These efforts are ultimately followed by a general relaxation and a temporary shift of the visual attention away from the details of the niche. Attention soon returns to the general situation again, and the second visual perception of the stylus in the niche is followed by more attempts to move in the wrong direction and then there comes a reflex reversal of the first movement. The following introspections are typical descriptions of this phenomenon, taken from recess period practice, speed and accuracy instructions.[14]

---

[13] Those of our learners used chiefly for introspection have been called 'observers.'

[14] It is to be regretted that the majority of our introspections are taken from recess period practice. This state of affairs is made necessary in the early part of the learning, since it is not wise to accumulate circuits at a time when so much occurs in the experience of the subject and when each circuit requires so much time for its completion.

Obs. 3 (1st day of practice). "As soon as I heard the signal I proceeded to follow the arrow." (The arrow indicated the direction to be followed at the beginning of the circuit). "Then my stylus struck the niche up on the twelfth side. I did not know I was moving in that direction until I struck the side; I tried to push away but found I was pushing against the side.

I attempted two or three times to get away but could not; I thought the electric current must be holding the stylus against the side; I was aware of intensive strains in my arm and the whole affair was very unpleasant. Fnally I gave up and relaxed for a time. I again looked at the star and noted that my stylus had moved partly out of the niche during my relaxation. I attempted to get away, but drove the stylus back into the niche again. Then the muscles of my arm and shoulder tightened and, with a quick movement, I pulled the stylus out of the niche and down past the arrow. This movement carried me again into the niches on the first side."

Obs. 6. (1st day of practice). "I got into a difficulty as soon as I started; I tried to get out by moving first one way and then the other, but this did not help. I was then aware of strains in my hand and arm; these strains became more intense, the more I tried to get out. Finally I moved right out of the niche. I did not direct this last movement; I simply saw that I was moving in the right direction. After this movement, whenever I saw I was going in the right direction, I refrained from interfering with the movement. While the strains were present and I was attempting to get out of the niche, I was aware of the following in vocal-motor imagery: 'Go in the opposite direction to the way it seems.' But I found that this did not help, for I did not know how to go in the opposite direction, until the new movement came mechanically."

Obs. 2. (1st day of practice). "I was first aware of a keen visual fixation of the groove and of the tip of the stylus. Then I observed that I was in a niche; I pulled out mechanically. When the stylus was going in the right direction I simply let it go; whenever I got caught I tried to get the kinaesthetic feel of the movement in the right direction, but I could not initiate this new movement. I simply kept on the lookout for the successful movements when they came. On the sixth side I got into a difficulty; I turned away from the mirror and was aware of the strains in my arm. When I looked back I saw I was moving in the wrong direction; this movement was stopped mechanically. Then I was aware of moving in a direction at right angles to the wrong movement; this new movement was right. When I am in a difficulty I am always aware of a focal visual fixation of the sides of the star and of the tip of the stylus."

Many difficulties of the same sort as those we have just described persist for many circuits of the star; the behavior is much the same as shown above, although the experiences eventually become short and fleeting. The following are illustrations of these difficulties which come up in later circuits of the star.

Obs. 1. (10th circuit). "When I get into a difficulty I am aware of manual-motor strains; the forward movement, or the one which is leading me into difficulty, is stopped mechanically. I become aware of a clear visual fixation of the groove, and then the successful movement comes. I find no important difference between my present method of meeting a difficulty and the method which I employed at the outset of the learning. The only difference I am able to observe is that the successful movement comes sooner, I do not wait so long. Formerly, in case of difficulty, the new movements were very short and I stopped momentarily after each one." (Do you pre-image your way out?) "No; I never do on the difficult sides; I simply look at the sides and sometimes move my eyes along the path, and the appropriate movement comes. The corners are most difficult for me, and, at these points, I am aware of clearly fixating the groove. On approaching a corner I immediately slow down the last movement, and the new movement along the next side comes mechanically."

Obs. 7 (2nd circuit). "When I get into a difficulty I move much more slowly; I am not aware of any conscious processes directing my movements. I have a clear visual awareness of the groove and the tip of the stylus. When I get out of the difficulty I am aware of visually fixating the plug ahead; this is followed by a rather quick movement down the groove."

Obs. 4. (5th circuit). "I cannot describe clearly my method of guiding the stylus in the difficult places. I seem to stand by as a spectator and watch my hand doing it. When I am on the fourth side, which is the most difficult side for me, I am aware only of clearly fixating the edge of the groove. There are intensive strains in my arm and hand, and the whole arm seems to be stiff and to work as a unit."

Obs. 7. (10th circuit). "At times I am aware of making the movement of tracing a difficult side with muscles in arm and hand somewhat relaxed; at such times the movements come easily. By this I mean that the strains in my arm are much less intense and the movements are faster. This is the only change I am able to observe which has taken place on the difficult sides since my first circuit."

Our introspections clearly show that the learning begins by trial and error and continues throughout on this basis. The only change which takes place through practice is the partly relaxed condition of the muscles as described in the foregoing introspection. We have no evidence of any other factors in the current control of the stylus than visual perception, followed by muscular strain which may be unpleasant. One of the strongest confirmations of this point is the fact that our observers, throughout the course of the learning, insist that the control comes entirely through the close visual fixation of the groove immediately ahead of the stylus point.

Obs. 3. (2nd circuit). "Most of my movements on the difficult sides are of the zigzag sort. The sight of a niche ahead serves to stop the movement mechanically." (5th circuit.) "All the wrong movements are inhibited by a downward pressure of the stylus; I simply see the stylus approaching the edge of the groove and I press down reflexly. If I watch the stylus tip and the groove carefully, my stylus moves down the center of the path and does not take a zigzag course as it does when I am not attending closely to the visual situation; formerly I zigzagged from side to side and frequently came into contact with the edge. Now I keep my eyes fixed throughout on the edges of the groove."

Obs. 4. (2nd Circuit). "I stop all the unsuccessful movements mechanically. I simply concentrate my visual attention and watch the groove. When I am watching carefully, the unsuccessful movements are inhibited before they touch the edge of the groove. There are intensive strains in my arm when these movements are inhibited, and I am aware of having pressed down on the stylus when the movement was stopped. When I am aware of being in a niche, I pull out of the niche reflexly."

Obs. 6. (5th circuit). "I inhibited all erroneous movements by pressing down on the stylus; the sight of the stylus moving toward the edge is followed by this reflex pressing down. I simply do it as mechanically as I wink my eyes when something is flying toward them."

Obs. 2. (5th circuit). "I check all the false movements mechanically. I clearly fixate the groove when the errors come, and this is followed by a tightening of the muscles which results in a stiffening of the forearm."

Obs. 9. (5th circuit). "All forward movements in the wrong direction are stopped mechanically; I am aware simply of staring at the sides; then muscular tensions develop and I am aware of pressing down on the stylus when it nears the edge of the groove."

Throughout the above introspections, secured in recess period practice, it will be seen that the observer emphasizes the mechanical inhibition of the forward movement. This is characteristic of recess period practice, speed and accuracy instruction, under conditions when the subject is selecting either speed or accuracy—in this case the latter—but cannot improve in both elements of the learning simultaneously, see Fig. 6, p. 16. In later series practice tracings, the mechanical, jerky movements give way to steady, flowing movements and the observer is unaware of the abrupt inhibition of forward movements.

b. *Difficult and Easy Sides.*—As was pointed out above, the sides of the star readily fall into two classes,—difficult and easy. The difficult sides may or may not be those which involve many errors or contacts between the stylus and the edge of the path. A careful comparison of the subjective estimate on the comparative difficulty of the sides with the learning efficiency thereon, fails to reveal the fact that the basis of the estimate of difficulty is the number of errors made.[15] Introspective analysis shows clearly that difficulty always involves the presence of muscular strains or tensions. The sides which are invariably easy are the ones parallel to the median plane of the body, where the muscular tensions are much less and where the movements are longer and more sweeping. A side which is found to be difficult is always a side where improvement is taking place; an easy side is, in the large majority of cases, one where little of importance takes place and where no improvement is being made. The oblique sides are often reported to be easy; and when so reported the error and time records show no improvement. The following typical introspections, taken from series practice, speed and accuracy instructions, illustrate the foregoing points:

Obs. 2. (3rd circuit.) "On the difficult sides today it seemed that there was a tendency for my hand to move in the wrong direction; and there was a very noticeable sensation of strain when these movements were inhibited. At these places where strains occurred I am aware always of fixating focally the edges of the grooves. Sometimes I find these difficult places somewhat unpleasant. Usually, in the difficult places, I stop, make tiny excursions, and when the correct movement comes it is allowed to complete itself."

Obs. 2. (5th circuit). "On the seventh side, which is the most difficult one for me, I was aware of intensive muscular strains in my hand and arm; these strains were especially strong when I was making the zigzag movements. The strains in my arm came after I entered the groove on the difficult side; the situation seemed to be clearly objective on this side. I was aware of fixating the tip of the stylus and the edges of the groove most of the time." (The tracing of this side showed great improvement of this circuit).

---

[15] To facilitate the matter of determining the efficiency on the difficult and easy sides, apparatus *I* was modified by placing copper plugs in the path at each vertex; with this variant we obtained the time and error data for the tracing of each side.

Obs. 2. (7th circuit). "The seventh side was difficult for me today. There was great effort there to avoid errors; this effort consisted in a very slow movement when I found myself near the sides. When the new movement came, I speeded up automatically if it went in the right direction; if the movement went in the wrong direction, I was first aware of stopping, and then of strains in my arm. There were no mental processes between the seeing that I was going wrong, the stopping of the bad movement, and the change of the new movement. The whole tracing on this side was slightly unpleasant." (The objective records show that the tracing on this side has improved in both time and errors).

Obs. 2. (8th circuit). "On the seventh side today I was aware of making little movements and also of visually fixating the tip of the stylus. This has become my procedure on all difficult sides. I do not set out with any purpose to do this; I merely am aware of sensations of muscular strain and these were slightly unpleasant." (The tracing on this side shows improvement in this circuit).

Obs. 2. (9th circuit.) "The seventh side was easy today. I think I went faster there; before I had completed this side my eye had gone to the next, and I was aware of muscular tensions in anticipation of it. There was a slight relaxation of the muscles of my arm on the seventh side and I did not fixate the edges of the groove so carefully as I do at times." (The seventh side today shows an improvement in time and an increase in the number of errors).

Obs. 3. (3rd circuit). "The sight of the grooves on the sixth side seemed to slow the forward movement of my stylus. I had automatically speeded up on the fifth side and was going at a good speed when I became aware of the sixth side. The visual perceptions of it seemed to turn my stylus reflexly into the groove of the sixth side. The first movement I made was wrong; my visual perception served to inhibit this movement. I was then aware of little short movements which were rather aimless; then a successful movement came and I allowed it to complete itself. There were intensive strains in my arm on this side and the tracing was somewhat unpleasant. I carefully watched the edges of the groove." (The tracing today showed great improvement).

Obs. 3. (4th circuit). "I had difficulty with the sixth side. I saw the stylus was about to touch the edge; this served to stop me reflexly. My stylus moved to either side in a zigzag fashion and was stopped when I was about to touch the edge. I was then aware of a movement down the middle of the path,—which seemed to come of itself. This movement carried me along the side. There were intense strains in my hand and arm, and the tracing was somewhat unpleasant." (The tracing shows improvement in both time and errors).

Obs. 3. (10th circuit). "On the sixth side I was aware of some short movements. However, I was not aware of very much strain during the circuit of the sixth side. The eighth, ninth, and tenth sides were also very easy for me today." (The sixth, seventh, eighth, ninth and tenth sides all show a loss in accuracy during this circuit).

Obs. 6. (8th circuit). "I do not anticipate the difficulty of a side until I begin to trace it, but I am always aware of the difficulty of a side after I pass it; the basis of the difficulty of the sides seems to be the muscular strains that were there."

The reader should note that the stylus always pursues a zigzag course in the earlier tracings of a difficult side. This course, as our objective and physiological analysis will show, is modified by the effect of the recess intervals into a course which is practically a straight line extending down the middle of the path. Thus it is seen that, in the earlier tracings, the forward movement of the stylus is accomplished by means of alternate right and left lateral[16] movements with a general forward trend; while in the later tracings, the forward movement is secured by means of a simultaneous functioning of those muscles concerned with the lateral movements, together with certain other muscles which produce the forward movement. It is this simultaneous functioning, or functioning in close succession,[17] of opposed musculature which gives rise to the 'stiff' arm, reported by all our observers attempting to secure accuracy under the conditions of recess period practice; and in our opinion, it is the 'stiff' arm which affords the physiological basis for the feeling of difficulty, always somewhat unpleasant.

The unpleasant muscular tensions occurring on a difficult side are followed by a marked relaxation, when the tracing of the side is completed; especially is this the case, when the difficult side is followed by an easy side. And this relaxation is the physiological basis of the pleasure which results from the successful tracing of a difficult side. This 'satisfyingness,' as Thorndike (49) would term it, is, in our opinion, nothing more than an indicator pointing to the fact that the tensions and strains on the difficult side are ended and thus they mark the transition from one physiological state, tension, to another state, relaxation.

---

[16] By lateral movement, we mean any deviation of the zigzag movements away from the general trend of the forward movement. Thus, if the general trend of the forward movement is toward the right, then up or down deviations from this forward trend would be termed lateral movements.

[17] The reader is referred to the following section for a detailed analysis of the physiological aspect of the problem.

c. *The Influence of Satisfyingness upon Improvement.*—Our introspections show that the successful tracing of a difficult side is very often followed by a feeling of satisfaction.[18] This feeling, in our opinion, results from the muscular relaxation which attends the completion of a difficult tracing and has no effect whatsoever upon the learning; while Thorndike makes the feeling of satisfyingness of prime importance in the selection of the successful responses. The following introspections will show the occurrence of this affective state in tracing the star:

> *Obs. 3.* (5th circuit). "As I approached the third side I had a faint auditory-motor *Aufgabe* 'go slowly around corner.' This was carried out and I moved around the corner in a straight line,—the usual zigzag course of the stylus was absent this time, which gave me a definite feeling of pleasure. On the third side I came near touching the side; this was followed by very slow movements which led me along the side without any errors, and was very pleasant."

> *Obs. 2.* (4th circuit). "On side one the first move the stylus made happened to be the correct one. There was no tendency to swerve from side to side and I completed this side without an error; this was followed by a marked relaxation and a feeling of pleasantness."

We are quite unable to see that this satisfyingness which follows the satisfactory tracing of a difficult side, or the making of a successful stroke, can in any way influence the reappearance in later practice of those responses preceding or concomitant with the affective state. That the movements followed by relaxation (satisfyingness) will often be repeated more or less in their original form is assured by the fact that they were fully completed,—a state of affairs further attested by the fact that they call forth no further strains or effort in the reflective memory of the observer during his act of introspection.[19]

---

[18] The reader is referred to Thorndike's Original Nature of Man, Educational Psychology, Vol. I, for a good account of his view of the part played by the feeling of satisfaction in learning.

[19] Joseph Peterson, in a recent article, (35) has ably supported this same contention; also John B. Watson, Behavior, Henry Holt, New York, 1914,

It is the progressive modification of movements that conditions improvement in this investigation, not the 'stamping in' of successful responses, as in Thorndike's view. This is clearly shown in our physiological analysis of the learning. On those sides of the star (the vertical sides) where every movement is followed by the feeling of satisfyingness, practically no improvement takes place from the first circuit to the end of practice; the improvement occurs on the difficult, oblique sides.

In the earlier stage of the learning the highest rate of improvement we have observed in this problem has occurred where the subject is given accuracy instructions and recess period practice of one circuit per day, or with recess of twenty-four hours. Under such conditions the improvement comes about wholly through the resolution of a zigzag course of the stylus into a straight course down the middle of the groove. We have already tentatively shown in our summary of the learning curve, which will be supplemented by other material in a later section, that the recess period is responsible for this change in the course of the stylus. The recess period resolves a zigzag course with marked lateral movements into a smooth course with no lateral movements; from which effect there results often a marked improvement in speed and accuracy in the first repetition after the recess.

From such evidence it is readily seen that no selecting agent, such as the 'satisfyingness of a response,' is operating to 'select out' the successful responses made in a series of random movements—the basis of the Thorndike view, since the successful responses, straight course of stylus, did not occur at all in the tracing before the recess. An entirely new movement has come about, the result, as we shall show in the following section, of the simultaneous functioning of opposed muscles which, prior to the recess, functioned in succession. The learning takes place without selection and no selector is needed,—the recess period alone being adequate to produce such physiological changes as will lead to the improvement.[20]

Chap. VII, points out that the known laws of frequency, recency, etc., are quite adequate for fixing arcs in habit formation.

[20] Thorndike's view has been formulated wholly to fit the case of improve-

D. Typical Behavior under Instructions

We have already touched upon many of the features characteristic of an observer's behavior when avoiding errors, or when speeding up; we shall now go into some detail in an analysis of this behavior. Observers were given the two types of instructions at various stages of the learning, one type insisting upon speed, and the other upon accuracy. Introspections were secured from all of our observers working under these two types of instruction; but we made a special attempt to obtain a more accurate analysis from two of our observers, 2 and 9, who possessed a high degree of skill in introspecting, and we shall select our citations largely from their introspections.

a. *Behavior under "Accuracy" Instructions.*—The most characteristic feature of the learner's behavior under these instructions is his clear fixation of the edges of the groove. When instructed to avoid errors, he immediately begins to fixate the groove; and, during the fore-period, becomes aware of strain sensations and of incipient movements along the first side of the star. (We have called the fore-period that period of time which intervenes between the instructions and the beginning of the tracing of the circuit). The fore-period is characterized by a great variety of imaginal and affective content, which, however, disappears when the movements of tracing are in progress; there remains merely the clear visual fixation of the edges of the groove, and the strain sensations from the muscular responses to the objective situation, with a high degree of affective toning. The following introspections will illustrate this point.

Obs. 9. (4th circuit.) Fore-period: "On hearing the instructions I was aware of a keen physical alertness; and before I looked at the star there was a mass of visual imagery; prominent among these visual images was that of a record with arrow marks upon it. Then I perceived the star; my eyes rested upon the starting-point for an instant, then moved down the side.

---

ment after many repetitions of an act, which is clearly evidenced by his extreme emphasis upon 'use' in the formation of bonds. His strictures against Book and Swift (Educ. Psychology, Vol. II, p. 314) furnish still further evidence that he is unable to deal sympathetically with those who would put any improvement upon the basis of cerebral growth or 'automatic organization of bonds.'

The motor side of consciousness became intensive, and I was aware of an incipient tendency to move my hand along the groove; this consisted of kinaesthetic sensations in my arm and shoulder. The muscles were tensely contracted in the shoulder and upper forearm." *Beginning of a Circuit:* "Your signal, together with the readiness above referred to, was the immediate antecedent to my starting off. At first there was just a visual percept of the movement; then I glanced at the plug ahead and was aware of a movement around the corner. As I turned the corner I was aware of clearly fixating the niches, my line of regard being just in front of the tip of the stylus. When I passed the corner and came to the vertical side, attention shifted to the muscles of my arm; I was aware here of the ease and lack of strain in the movements and was also aware of speeding up reflexly. My visual line of regard was much farther ahead of the stylus when I was moving faster. When I came to the eighth side, I perceived the whole side at one glance; this was an easy side and I made it in one stroke. On the ninth side I was aware of a dull pain in my shoulder and arm as the stylus moved along; I fixated the niches of the side and my control seemed to come entirely from this visual perception." (5th circuit, no recess period between 4 and 5). "This circuit was much less difficult than the former; I gripped the stylus less tightly, moved faster, and my visual attention was directed much farther down the groove."

Obs. 9. (6th circuit, after recess). *Fore-period;* "I had vocal-motor images: "Reduce errors.' Then I was aware of clearly perceiving the groove; this was followed by gripping my stylus. There was an increasing tension in my arm and shoulder." *Beginning of the Circuit:* "On the signal I was aware of a keen visual fixation of the edges of the groove, and the movement of the stylus down the groove. I also noted a sense of heaviness in my muscles as I approached the corner; this seemed to be chiefly a dragging sensation in my arm and shoulder, and was always a characteristic of my accuracy *Aufgabe*. At the corner there was also concentrated visual attention on the niches. As I proceeded around the star I was aware that my visual line of regard was progressing farther and farther from the tip of the stylus; I was also aware of moving faster at such times. This awareness of speed was followed by an increased pressure on the stylus, which reduced my speed."

Obs. 9. (7th circuit, no recess between 6 and 7). "In this circuit there was much less tension in my muscles and my visual line of regard remained much farther ahead of my stylus than in the first circuit. I was aware of moving faster and faster as I proceeded. At times I was aware of a visual perception which seemed to include the whole star; and at such times my speed was very marked. After getting caught in a niche I was aware of the *Aufgabe* to decrease my errors; this consisted of a memory image of rapid movement in previous circuits with visual and kinaesthetic images of errors. All of this developed into a keen concentration on the groove immediately in front of my pencil point; there was also an awareness of increased pressure on the stylus, and a stiffness in the muscles of the arm and shoulder."

In the above introspections we have an unusually clear account of the gradual development of speed under series practice conditions, which, in an earlier section of this paper, was shown to take place when circuits are accumulated without intervening recess. (See Figs. 2 and 3.) A striking characteristic of the development of speed through series practice is the fact that visual attention spreads over larger and larger areas of the star. In recess period practice the visual perception is narrowed down to the tip of the stylus and the edge of the groove on either side of the stylus. Another significant point in the above introspections is this observer's clear analysis of his accuracy *Aufgabe,* which consists simply of a close visual fixation of the objective situation, with pronounced muscular strains in the arm and upper trunk. The follownig introspection is significant because it reveals the extreme emphasis upon accuracy after a long recess.

Obs. 9. (8th circuit, two weeks after the foregoing circuit). *Fore-period:* "I was aware of vocal-motor imagery: 'Decrease error.' Then I had a visual image of seeing my hand go around the groove; I was aware of a gradually increasing tension in my arm and shoulder, and became impatient to begin. There were incipient movements of beginning." *Beginning the circuit:* "As I began there was a very close concentration of visual attention on the center of the grove; the movement was slow and there was a marked muscular strain; at the corner I gripped the stylus tightly and watched the niches carefully. On the difficult sides in this circuit I was aware of great muscular strains; it seemed as though I were pushing against a wall, in fact, I actually visualized a wall at such times. On the eighth side I was caught in a niche, this was followed by an auditory image, 'accuracy,' in terms of your voice; then I was aware of the return of the accuracy *Aufgabe* which I have already described."

The following introspection furnishes a more complete analysis of this observer's accuracy *Aufgabe.* The observer had been asked to seat himself before the star and then to wait for the instructions.

Obs. 9. (9th circuit). "When I sat down before the star I was aware of a gradually increasing adjustment to the total situation. This consisted of muscular strains of sitting up straighter in my chair, glances at the star, and at times a focal visualization of the starting-point. Then after your instructions I repeated them to myself in vocal-motor imagery." *Beginning of Circuit:* "On the signal there was a sudden rise in the focal intensity of the visual perception. There was no kinaesthetic image of movement; I was simply aware of moving down the groove with my eyes fixed on the tip of the stylus and upon the edges of the groove on either side. I was often aware of the instructions in auditory imagery, which was followed by a clear visual awareness of the groove at the tip of my stylus, and then I was aware of strains in my arm and shoulder,—the arm seems to be quite stiff. All this constitutes my accuracy *Aufgabe.*"

The reader will observe that throughout all the introspections quoted, there is one element of control,—the clear visual perception of that portion of the groove which lies immediately in front of the stylus. As speed develops, the line of visual regard moves away from the stylus and the visual perception seems to be spread out; when a difficult place is encountered, the visual line of regard returns to the tip of the stylus. This is often followed by an imaginal representation of the instructions. All of our introspections show, however, that this representation of the instructions does not appear in advance of the strain sensations which are characteristic of the accuracy *Aufgabe.* In the vast majority of our introspections, the verbal instructions do not appear after the tracing of the circuit has begun.

We shall now quote a typical introspection of Observer 2. The practice here was under the accuracy instructions.

Obs. 2. (3rd circuit). *Fore-period:* "I was first aware of a remembrance of the last circuit. I had a visual image of holding my stylus in a slanting position; I saw the contact on the plug and the electric spark. This was followed by an adjustment of the stylus in which I raised it to a perpendicular position." *Beginning of Circuit:* "On a signal I started out mechanically; the first movement happened to be right. I was aware of intense and shallow breathing; there was a tension about the face, jaws, and a gritting of the teeth as I approached the corner; this was concomitant with a clear visual awareness of the corner. Throughout this whole circuit I clearly perceived the groove just in front of my stylus. Sometimes there were movements in the wrong direction; these were inhibited mechanically. When I was not under strain I noted that the speed of my movement had increased; when I was aware of speed my line of visual regard was always far ahead of the tip of the stylus."

b. *Speed and Accuracy Instructions.*—Under this type of instructions we have a somewhat different behavior, yet one that is identical with that which obtains in the speeding-up places under the *accuracy* instructions. In the *speed and accuracy* instructions the observers were instructed to go around the star as fast as possible and avoid errors as far as possible. It should be noted in these introspections that we have an occasional reference to the use of kinaesthetic control image.

In the following introspection the observer was attempting to secure speed and accuracy under recess period conditions; this leads to an alternation between the speed and accuracy attitudes and involves an extraordinary amount of muscular strain and feeling of effort on the part of the observer.

Obs. 2. (15th circuit). *Fore-period:* "When speed and accuracy instructions were given I was aware of clear kinaesthetic strains in my shoulder, and there was a very strong tendency to start before the signal came. There was great muscular tension throughout my body." *Beginning of Circuit:* "I began with my attention on the kinaesthetic situation. There were strains in my arm, shoulder and back. I had a kinaesthetic image of movement down the first side; this was followed by the movement. I was aware, as I proceeded, of increasing muscular strains: these were localized in the muscles of the back and arms. There was a rapid increase in the rate of the heart beat. I made many errors and was caught a number of times in the niches; I went much more slowly after being caught. On the whole, the last half of the star was traced more slowly than the first, and my attention was more on the niches and the general visual situation, and less on the kinaesthetic elements. When I reached the starting place I observed that I had gone around the star the wrong way."

In the above introspection it will be observed that, during practice in speed, the learner's attention shifts from the visual to the kinaesthetic situation; this we observed to be the case in the accuracy practice whenever speed was developed at the easy places; after the errors were made, however, when accuracy became necessary, attention shifted back to the objective situation. It should also be noted that the learner began with a kinaesthetic control image which is the first to be reported so far. The observer also traced the star the wrong way; that is, he began with the twelfth side and ended with the first side. (If the reader will turn to Fig. 1, he will note that the twelfth side extends upward and is in the apparent direction of the first side; in other words, the mirror image simply reverses the relative position of the twelfth and first sides).

The two following quotations are presented here because of the light they throw upon the working of a plan of procedure. These two were chosen from a host of others, all of which bear out the same points, namely,—that no conscious use of a plan or device is of value in tracing the star, except the plan of tracing deliberately. The experienced subject starts out slowly and moves deliberately, even though he be instructed to trace at his highest speed; this we have called the *discounting* of the speed instructions. However, this discounting of the instructions occurs only when the subject is working under speed and accuracy instructions.

Obs. 9. (40th circuit). *Fore-period:* "Before the instructions were given, I was engaged in working out a plan of procedure. I decided to hold the stylus vertically at all times and grip it tightly." *Beginning of Circuit:* "As soon as I heard the signal there was a rapid plunge along the first side. My eyes were fixed on the objective situation; the movements were very mechanical; there were strains in my arms and in the muscls of my shoulder and back. At times I speeded and at times I went very slowly. At the speed places my eyes were directed far ahead of the stylus; but at the difficult places I perceived clearly the tip of the stylus and the edges of the groove on either side. My stylus was caught a number of times during the circuit and I found that I was unable to make use of my plan of procedure. It seemed that the objective situation determined my reaction."

Obs. 2. (20th circuit, 5th circuit under the special caution to introspect her plan of procedure). *Fore-period:* "In this fore-period I was aware of working out a plan of procedure. I first had visual images of moving very rapidly around the star without touching the sides; then I had a visual image of getting caught. This was followed by intense unpleasantness and vocal-motor imagery, 'you had better go slowly at first.'" *Beginning of Circuit:* "On the signal, my visual attention narrowed down to the tip of the stylus and the groove. I started in with slow movements, but on the fifth side my visual attention moved ahead of the stylus and I was aware of speeding: the last half of the circuit was made at much higher speed." (Was your plan of procedure successful?) "Yes; this one was. I find now that when I begin slowly and attend to accuracy, I make higher speed than when I begin impulsively."

c. *Positive and Negative Instructions.*—An attempt was made to study the effect of positive and negative instructions upon the tracing of the star diagram by mirror image. Our quantitative data and introspective accounts furnish no support to Langfeld's (23) finding that only negative instructions suppress the control imagery. In truth, we find that under both positive and negative instructions all imaginal processes are absent *during the tracing.* When the subject reaches an easy place on the diagram he reflexly increases his speed of movement and may report the presence of imaginal processes at such times. The difficulty encountered in tracing a side determines whether consciousness must deal focally with the objective situation, or be left free to entertain imaginal processes. Whether the subject is instructed to "avoid touching the sides" or "go down the middle of the path," the quantitative results were the same in our experiment.

In a number of trial tests we employed the Stoelting tracing board used by Langfeld, and our results were able to show that the quantitative findings secured by him could be rendered more decisive, and be clearly explained, by controlling the recess period, which Langfeld fails wholly to do.

E. The Question of Control Imagery

We were surprised by the lack of control imagery in our introspections. Our observers had invariably reported an abundance of imagery in the fore-period; but this imagery appears to drop out during the tracing, especially so under accuracy instructions. In isolated cases our learners reported that a manual-motor-image was present before they began to make a rapid stroke down an easy side, but that this image dropped out during the course of the tracing. It occurred to us that the control might possibly be furnished by the visual perception in cases where the circuit was traced slowly under accuracy *Aufgabe,* and that a kinaesthetic or manual-motor image[21] might be the means of control in cases of high rates of speed. In order to test this conjecture we arranged the following series of experiments:

a. In a first experiment, Observer 5, who was about to begin her fifth circuit of the star, was instructed to begin with a rapid movement along the first side. It was thought that if the kinaesthetic image should appear at all during the tracing of a side of the star, it would tend to appear under such instructions. A quotation from her introspection follows:

---

[21] Throughout our analysis we have considered that the kinaesthetic image of making a movement is the same as the manual-motor image. In both cases, our observers have always found a visual component, i.e., in a kinaesthetic image of making a movement down a groove, the 'down the groove' meaning is carried by a visual image of the groove. Whenever we have desired to keep clearly before the reader the visual and the kinaesthetic components, we have resorted to the use of hyphenated words, visual-kinaesthetic or visual-manual-motor, in describing the imagery.

> "I was first aware of my *Aufgabe* in auditory-vocal-motor imagery: 'hurry and make no mistakes.' Then I began the first movement with a quick manual-motor image of movement down the first side of the star. I was dismayed when I saw my hand going along the twelfth side instead of the first side. I stopped the movement and went slowly along the first side, but this time I had no image of the movement."

Seven days later this observer was given the same instructions and her introspection reveals precisely the same state of affairs. This situation, together with the introspection of Obs. 2 on her fifteenth circuit, as above noted, was sufficient to lead us to doubt the ability of the kinaesthetic image to function as a mode of control in this problem.

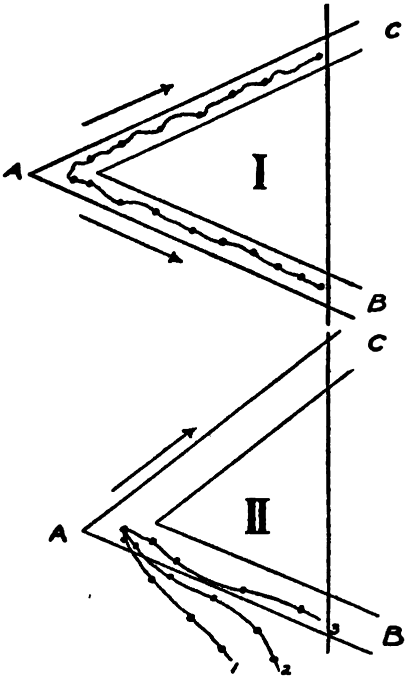


FIG. 9. An exact copy of the results of an experiment performed with Subject No. 9 upon apparatus II. In *I* the subject traced the path *AB* slowly and deliberately, and likewise the path *AC;* in *II* the subject was instructed to trace the side *AC* indicated by the arrow, with one rapid stroke. In the three attempts at tracing in *II,* the subject traces in the apparent direction of the path *AC.* The stations on the tracing line indicate the time in fifths of a second.

b. To test the matter more completely, we added another special experiment, which was arranged as follows: Two sheets of blank paper were pinned together and a sheet of carbon paper was inserted between them. A diagram in the form of a simple acute angle was drawn on the upper sheet, its carbon impression being recorded on the sheet below. (Fig. 9). This diagram was placed upon the tapping machine, Apparatus II, so that the sides of the angle were in exactly the same position as the first and twelfth sides of the star. Observer 9 was chosen as observer; since this observer had made about one hundred circuits of the star prior to this test. The time-recorder was set at five beats per second. The observer was asked to start at *A* and trace the side *AB,* Fig. 9, I, and to keep directly in the middle of the path and move as fast as possible; this was our speed and accuracy instructions. His introspections revealed the presence of no control imagery. He was then asked to trace the side *AC* under the same instructions; and control imagery did not appear here.

In a second test, Fig. 9, II, the observer was asked to trace the side *AC.* keeping as nearly as possible on the line, but 'to make the tracing with one rapid movement.' The observer was aware of a kinaesthetic control image in this tracing; his stroke, however, was on the side *AB* and is shown in Fig. 9. He was asked to repeat the stroke a number of times. The time of each stroke is indicated on the path of the stylus, (Fig. 9), and it should be noted that the fast strokes move out on the apparent side and not on the side designated.

The detailed introspections of this observer reveal the fact that during the fore-period of the one-rapid-stroke test the observer was repeatedly conscious of manual-motor images of moving the stylus down the appropriate side. He assured us that these images could be reproduced at will and he was confident they were adequate to control the movement. However, when the stroke was made according to the particular instructions which were best suited to the production of the image,—one rapid-stroke,—he traced the apparent side of the angle rather than the true side, as in the case of the two former experiments. (The reader should note that in the mirror image the side *AB* is in the position of the side *AC* of the direct vision situation). On the repetition of this test, however, the subject traced the appropriate side slowly and deliberately, without the slightest trace of manual-motor imagery and with complete awareness that the control came from the visual situation.

Another phase of this observer's analysis of his behavior under instructions lies in the behavior of the visual attention. When tracing slowly and with deliberation, his attention is directed focally to the visual details of the line to be traced; but when he reacts under the one-rapid-stroke instructions, his visual perception consists merely of a general awareness of the direction of the path; and, in addition, there is a marked consciousness of the motor situation, involving muscular strains in the shoulder and arm. This latter is precisely the state of affairs we found in the speed tracings on the star, i.e., the definite awareness of the *motor* situation alternating with, and never concomitant with, the focal visual awareness of the objective details.

When the objective situation was removed and this observer was asked to reproduce the objective and subjective situations in imaginal terms, his analysis reveals that the essential character of the visual component is a certain lack of details,—the line to be traced is imaged as a whole; and, he introspects that the awareness of the motor component follows directly upon the awareness of the visual element. The observer is further convinced that the imaginal state of affairs in this instance is the exact replica of that which obtains in the fore-period when he is instructed to make the tracing of the line in one-rapid-stroke.[22]

Summarizing our findings in the case of the above observer, we find (1) he is able to follow the path designated by moving slowly, with clear visual attention upon the objective details, but is unaware of imaginal processes; (2) he is able to trace the proper line with one rapid stroke, provided he delays the start—our initial delay—by pressing downward on the stylus until he has inspected the visual details of the path, but is unaware of imaginal processes; (3) he traces the apparent line,—thus falling back to the first stroke he employed when he began mirror tracing,—when guided by a visual-manual-motor image, or when the visual objective situation is diffuse or general.

---

[22] It should be noted here that the trained subject is able to trace the proper line with one-rapid-stroke by delaying the stroke through pressure on the stylus until the eye has inspected the details of the line. This delay we have called the 'initial delay' and it is worthy of note that when the subject resorts to the initial delay he is unaware of imaginal processes but is conscious, with an unusual degree of clearness, of the details of the objective situation.

From the foregoing, we are led to the conclusion that in the case of this subject the visual objective situation—detailed visual awareness of line and visual awareness of the stylus moving along the line—is the sole means of control.

c. To further test the problem of imaginal control, an advanced class of twenty students in experimental psychology in the University of Utah was given the following experiment. A diagram similar to the one shown in Fig. 9 was printed on cards. The students learned to trace the line *AB* by repeated practice, a new card being used for each tracing. When each student had traced the design fifty times, and thus, for all practical purposes, had reached his limit of improvement, he was asked to draw the diagram, which he had not seen for twenty-four hours, and indicate the line *AB* which he had been tracing. Each student was required to furnish an introspective account of his procedure in making the drawing.

Seventeen of the twenty students drew the line in the position of *AC*, and the entire twenty were convinced that the mental basis for constructing the diagram was the visual image of the path, combined with the manual-motor image of making the movements, the manual-motor image being the stronger component. If this manual-motor image gives no adequate basis for correctly locating the path that has been repeatedly traced, it is inconceivable that it can function to control the movement of tracing in the original situation.

d. A careful examination of all of our introspective data secured from our nine trained observers reveals the fact that control, or antecedent, imagery is almost completely lacking in this experiment. By control, or antecedent, imagery, we mean that imagery of a movement which just precedes, in point of time, the execution of the movement. *Observer No. 1,* who has made over one hundred circuits of Apparatus I, under all varieties of conditions as to recess periods and instructions, was not aware of a single antecedent image during the entire practice. This observer is of a decidedly motor type and is highly trained in introspection. *Observer No. 2* reported two antecedent images during twenty-five circuits and both of these occurred during periods of partial relaxation or fore-periods preceding difficult portions of the star. *Observer No. 3* reported three antecedent images during twenty circuits of the star. *Observer No. 4* was not aware of antecedent imagery in two hundred circuits. *Observer No. 5* was quite frequently aware of what seemed to be antecedent imagery; this was also the case with *Observer No. 6.* *Observer No. 7* was not aware of antecedent imagery in making over one hundred circuits of the star. *Observer No. 8* reported no antecedent imagery. *Observer No. 9,* whose introspections have been used so largely in this paper, was aware of images of visual-kinaesthetic type only during fore-periods or during periods of relaxation; these, we have clearly shown, could not act as agents of control.

From the results secured in our special tests and from our general evidence, we are unable to see that control imagery can function in this problem. The fact is worthy of note that most of our observers expected to find control imagery; the fact that they did not do so became all the more interesting and important for our investigation. We have endeavored in this investigation to present all the available evidence fairly and impartially; but we have yet to find adequate evidence that a causal relation exists between the imaginal processes reported and the subsequent movements.[23]

---

[23] The writer, who has made more than one hundred circuits of the star, repeatedly sat as observer in the same experiment as was performed upon observer No. 9, above. The experimental findings and introspective results in our case are the same as reported for observer No. 9. The writer is able to get the clearest and most definite visual-kinaesthetic imagery of making the tracing with one rapid stroke; and yet, he cannot make the stroke unless the start is delayed by downward pressure on the stylus until the details of the path have been deliberately inspected visually; and in this case, there is not the slightest evidence or subjective feeling that the antecedent imagery was causally related to the movements. In fact, the writer is clearly aware, in such cases, that the imagery has no connection at all with the movements following the initial delay.

It is our belief that, in this problem, the illusion of movement produced by the mirror provides, for the problem of imaginal control, precisely the same analysis of the elements composing the complex, that the illusion in general does for the psychology of perception. In the former case it provides the most unmistakable evidence that the imagery antecedent to a movement cannot control the movement. The visual-kinaesthetic complex in imaginal terms is the precise replica of that objective visual and tracing situation in which a diffuse visual perception of a path to be traced is followed by a tracing in the apparent direction, rather than the true direction, of the path. The ***sine qua non*** of the visual imaginal situation,—since it is known to come quickly, and before the movement,—lies in its diffuseness. There is no new element in the imaginal situation which does not inhere in the perceptual situation; and thus, it cannot be a cause or a controlling agent for the movement.

Our conclusions for the introspective analysis may be summarized as follows: The behavior of the observer working under recess period practice with accuracy instructions, consists of a close visual fixation of ~~the details of the path to be~~ traced; slight ~~muscular strains~~ in the arm and shoulder develop because of the simultaneous functioning of opposed musculature,—the stiff arm of recess period practice; and there is an entire absence of imaginal processes of any sort. Under series practice conditions the visual attention comes to be spread out over a larger area of the star; the speed of tracing becomes greater, and the muscular tensions of the arm and shoulder in many cases becomes greater than for recess period practice.

Under speed and accuracy instructions, with recess period practice, the subject alternates between the accuracy attitude and the speed attitude. Under series practice, with speed and accuracy instructions, the observer maintains throughout the speed attitude which is made up of a diffuse, visual perception of the path to be traced, and fast movements, with the muscular strain much less than when the observer is attempting to secure speed in recess period practice.

In all accuracy practice the speed of the stylus is much the same

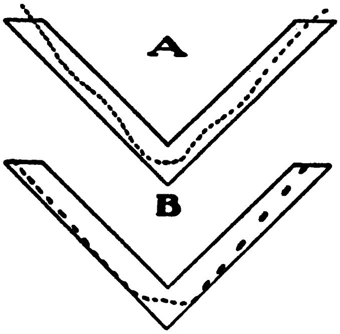


FIG. 10. An exact duplicate of a record made by Subject No. 3 upon apparatus IV. The tracing in *A* represents the work of the subject before he had attained a high degree of speed; the record in *B* was secured after the subject had acquired much skill and speed in tracing the diagram. The time is indicated in twenty-fifths of a second. In *A*, note that the time is spent equally on the two sides of the diagram; in *B*, where the subject is working under speed instructions, a great part of the time is spent on the first half of the diagram.

throughout the tracing; but in speed practice there are places of relaxation where the visual attention moves ahead to inspect the details of a difficult segment of the path; this is clearly shown by Fig. 10. During these periods of relaxation preceding the difficult tracings there may be, and often are, visual and manual-motor images of making the tracing, but these have been shown to be of no value in controlling the movements which follow them.

3. OBJECTIVE AND PHYSIOLOGICAL ANALYSIS OF THE LEARNING

Throughout our account of the physiology of the learning we shall adopt the "neuromuscular unit" conception as advanced by Stiles (43). While we shall not go far as to accept the extreme position taken by Keith Lucas (25) in the matter of the fractional functioning of muscles, yet we do contend that a musculature must be conceived of as functioning at times fractionally, and at other times, as a whole. The physiological basis of this fractional functioning would seem to be the neuromuscular unit. In the eye-hand coordinations under investigations here, the neuromuscular unit may be conceived of as beginning at the cortical centers and extending to and including the fibrillar units of the arm musculature.[24]

It is the central finding of this investigation that the number of efferent neuromuscular units to function simultaneously will depend upon certain conditions, the description and analysis of which constitute the chief aim of the writer in the following pages. The number of functioning afferent neural fibres extending from the retina to the cortical centers will be considered as constant; while the number of efferent fibres will be shown to vary.

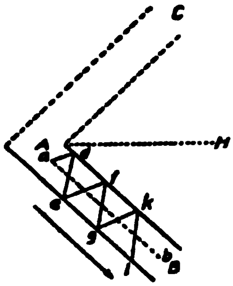


FIG. 11. A diagrammatic tracing of the first side of the star.

A. Function of the Recess Period

a. *Elimination of Zigzag Paths.*—We have already indicated that one aspect of the learning in this investigation takes place through the resolution of the zigzag course of the stylus into a smooth course: the recess period is the important factor in bringing about this resolution, since, without the recess, the only improvement possible is in the speed of the tracing. By referring to Fig. 11, it is clear that the smoothing of the zigzag path has come about through a fusion, simultaneous functioning, of the muscular elements entering into the successive movements, as in *ad* and *de*. That this fusion is not due to repetition, but wholly due to the recess period, may be seen by the course of the stylus in successive tracings which have not been separated by recess periods, Figs. 2, p. 13 and 12. If the recess periods are not inserted the zigzag course of the stylus is still retained.

---

[24] The writer believes that it is physiologically correct and probably more exact to conceive of the neuromuscular unit beginning at the retina, rather than at the cerebrum. According to the former conception, the findings to be reported in this investigation, as to the varying number of muscular units to function under different conditions of practice, would be described as a variation in the number of neuromuscular units functioning from the retina to the arm under different conditions of practice.

This view is entirely compatible with the findings of this investigation. However, the point is a technical one and has not, to my knowledge, been presented by writers on the physiology of the nervous system; consequently, we shall follow the easier plan of describing a variation in the size of a functioning musculature as due to a variation in the number of neuromuscular units functioning from the cerebrum to the muscle.

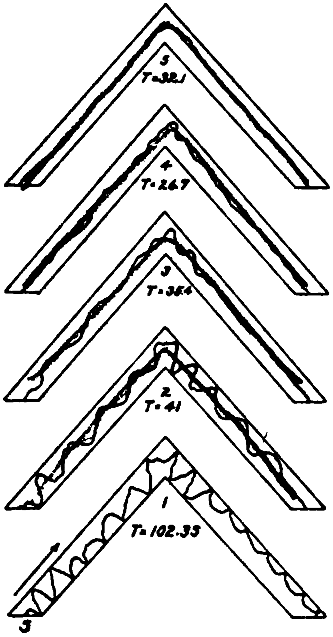


FIG. 12. An original tracing of a beginner, Subject No. 12, upon apparatus III. The circuits of the diagram were separated by ten minute recess intervals, excepting 3 and 4 which were in series. The time for the tracing of each circuit is indicated under the drawing.

b. *The Two Sources of Speed in the Learning.*—By referring to Fig. 13, it will be observed that this subject, who is tracing apparatus I, under recess period conditions with accuracy instructions, gains very much in speed of tracing, due to the elimination of the contacts against the edges of the path. The time lost in these contacts, and also that lost in the periods of arrest when the stylus is caught in the niches, is thus saved when the course of the stylus is straightened or smoothed out after the recess

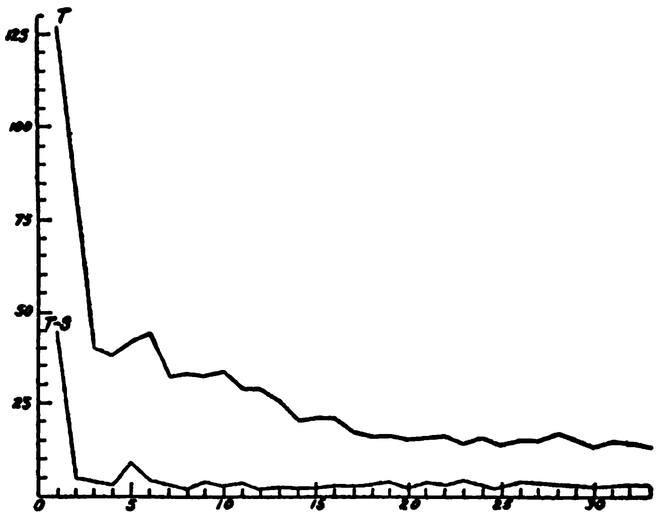


FIG. 13. A learning curve of Subject No. 8 which shows, in the upper curve, the total time for the tracing and, in the lower curve, the time actually spent with the stylus in contact with the metallic edges of the path upon apparatus I. The first three circuits were made under recess period practice and the remainder under mixed series and recess practice.

period; we have called this gain 'indirect speed' or the gain through smoothing the course of the stylus.

There is another type of speed which is brought about through series repetitions, Figs. 3 and 5; this we have styled the 'speed of facilitation.' In this type of speed the smooth course of the stylus does not vary during repetition, unless the speed be acquired too rapidly, as for example, under speed instructions, Fig. 16, p. 55. In the latter case the fusion and balance of the muscular elements is lost entirely and there results not the controlled movement down the middle of the path, but the successive movements which existed at an earlier stage of the learning.

In later pages it will be shown that, aside from indirect speed brought about through straightening the path, the recess period usually works to reduce the speed of movements which has been acquired through series practice. Series practice, through the speed of facilitation, will increase the speed of accurate movements whose accuracy has been built up through recess period practice.

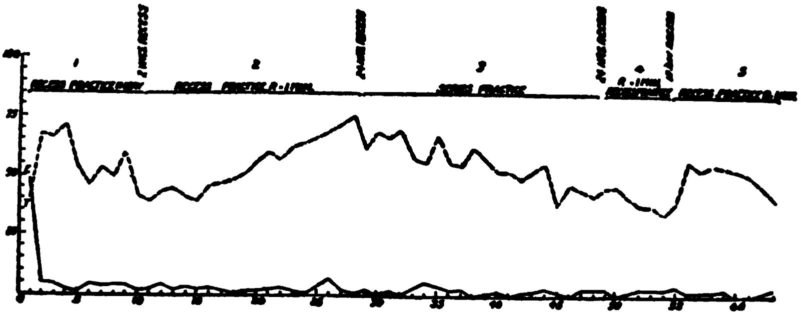


FIG. 14. Ordinate indicates time and errors; abscissa, successive circuits. This learning curve was secured from the work of subject No. 13; accuracy instructions were employed throughout. Part II of the practice shows a very marked irradiation curve. It should be noted that series practice in Part III brings about a marked improvement in speed without resulting loss in accuracy. The curve here shown is taken from data secured after the subject had completed a recess and a series practice, such as is shown in Part I of Fig. 2.

c. *The Irradiation Picture.*—The increase in time required for a tracing after the initial drop in the learning curve, as indicated by latter part of part I, Fig. 4, p. 14, or part II, Fig. 14, is closely paralleled by what we have styled the *irradiation picture.* This irradiation is clearly conscious to the subject, but, more important for our objective analysis, it is a matter to be observed by the experimenter; hence, it is essentially an irradiation picture.[25]. The irradiation begins with the very first circuit in part I of Fig. 4; but, one aspect of the irradiation—the increase in time required for tracing—is masked in the early part of stage I by the gain in indirect speed through straightening the course of the stylus, which it is the special function of the recess period to secure.

In the early tracings in stage I, the subject grasps the stylus much as he would a pen or pencil,—the functioning of the musculature concerned being characterized by a certain ease and facility; but, as the recess period practice is continued, the subject grasps the stylus with an observable stiffness; the staff of the stylus, which at first lay back on the hand, is now perpendicular to the plane of the apparatus; the arm, which was at first mobile, now becomes stiff and rigid; the upper trunk, which at first was inclined over the apparatus, now becomes stiff and erect; the opposed musculature in the legs becomes tensed, resulting in a marked rigidity and stiffness; apparently the last musculature to be added to this assimilation is that of the unused arm. This irradiation picture continues to become more pronounced as long as the time curves continue to rise. In the latter part of stage I of Fig. 4, the irradiation picture becomes so pronounced as to be of outstanding prominence to the spectator; this being the case also during part II of Fig. 14.

---

[25] Our conception of this irradiation, as will be shown in greater detail later, is that it is a physiological phenomenon characterized by a spread (irradiation) of nerve energy out over additional nerve fibres in the cerebrum. This becomes manifest to the experimenter as an increase in the musculature functioning in the act of tracing.

This irradiation picture is quickly terminated by series practice, or slowly terminated by long repetition under recess period practice, Fig. 4. The order of elimination of this musculature is just the reverse of the order of the appropriation, i.e., the last to disappear is the opposed musculature of the used arm, which was the first synthesis to be established in the recess period practice. So long as this elimination is carried on by series practice under accuracy instructions, the opposed musculature of the used arm retains the simultaneity of function; but, when the subject traces under speed instructions, this synthesis is lost, Fig. 16, and the stroke of the stylus takes the apparent direction of the path instead of the true direction, the acquired muscular elements having been completely eliminated.

While we are not especially interested in the economy of learning in this investigation, yet, Fig. 14 points to the fact that series practice under accuracy instructions, is the economic way, without a resulting loss in accuracy, to eliminate any appropriated musculature which happens to be unnecessary for the particular degree of accuracy desired for the tracing; thus, it is true that the irradiation to the back, legs, and the unused arm may be of great value for securing a perfectly straight course down the middle of the path, without the slightest deviation; yet, such a high degree of accuracy is unnecessary, and some of the musculature could well be eliminated.

After a high degree of accuracy, one or two errors per circuit, has been attained in the irradiation curve, Fig. 14, there is no change in the smooth course of the stylus; this is shown by the constant error curve. The same has also been shown by employing a star diagram on paper which the subject traced with a lead pencil; in this experiment, the course of the stylus is observed to remain unchanged from circuit to circuit. The changes taking place in the irradiation curve may be regarded as the addition of surplus musculature when the time curve is showing loss, and the elimination of this musculature when the time period is showing a gain. Fig. 14, part II, illustrates predominantly the function of the recess period, and part III the function of series practice.

d. *The Initial Delay.*—When the behavior before the star is reduced to its simplest elements, we shall see more clearly the part played by the recess period. In an experiment designed to simplify the response of the subject, we employed a highly trained subject to trace the diagram of our Apparatus IV under the instructions,—'Make one rapid stroke down the first side of the diagram.' This is the procedure we had adopted in a previous section in testing out the possibility of control imagery in the learning, page 40). Working under these conditions, the subject is able to make the stroke down the proper path only by delaying the movement until he has visually fixated the details of the path to be traced. This fixation requires a measurable interval of delay before the beginning of the movement, which we have called the 'initial delay.'

It should be noted that if the subject, even though he has had much experience in tracing this diagram, begins the first stroke of a series without this interval of delay, his stylus moves out on the apparent path rather than on the designated path, Fig. 9, p. 40. However, after the subject has once deliberately fixated the details of the path, he is able to make a number of strokes without the same amount of fixation, and hence with reduced initial delay, Fig. 15. The amount the subject may reduce this initial delay is largely a matter of individual difference; and depends upon the time interval between strokes. If the initial delay is reduced too rapidly, or to too low a level, the subject loses his control and his stylus moves out on the apparent side, Fig. 16.

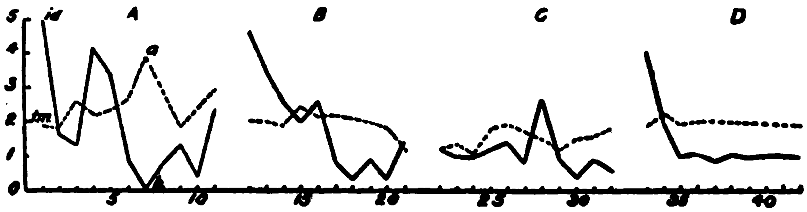


FIG. 15. A record of four experiments made with Subject No. 9 upon apparatus III. The path to be traced was the left half of the diagram as shown in Fig. 12. The instructions were: "Make the tracing with one rapid stroke." The initial delay is shown by the solid line and the time of the movement by the dashed line. The strokes were made in series and the subject selected his own time rate of making strokes. *A* and *B* were separated by a ten minute recess interval. The series *C* was made by direct vision, i.e., without the mirror; series *D*, with the mirror, was made after five days practice involving 200 strokes. The points *a* and *b* indicate a stroke which was made without any initial delay, the stylus moving in the apparent direction of the path and thus delaying the movement. The ordinate indicates time in seconds; the abscissa, successive strokes.

The points just noted are taken from results secured with a subject highly experienced in mirror tracing. From these results it is seen that the recess period works to bring about a marked initial delay which, as we have shown, involves a renewal of the long fixation of the details of the path; but, after the long fixation is made, a number of correct strokes may follow in series with greatly reduced fixation. Here it would seem that the neuro-muscular elements brought into function by the fixation, remain unchanged during a subsequent series practice; this we have called the *perseveration of the irradiation pattern*. It should also be noted that the recess period has little effect upon the time of the movement itself,—having its chief effect upon the initial delay or the period of visual fixation of the path to be traced.

e. *The Initial Delay and Perseveration of the Irradiation Pattern.*—From the results secured through the reduction of the behavior of the subject to the rapid tracing of one side of the star, as shown above, we are able to explain many of the points observed in our analysis of the learning curves in an earlier section.

We have observed that, after the initial drop in the learning

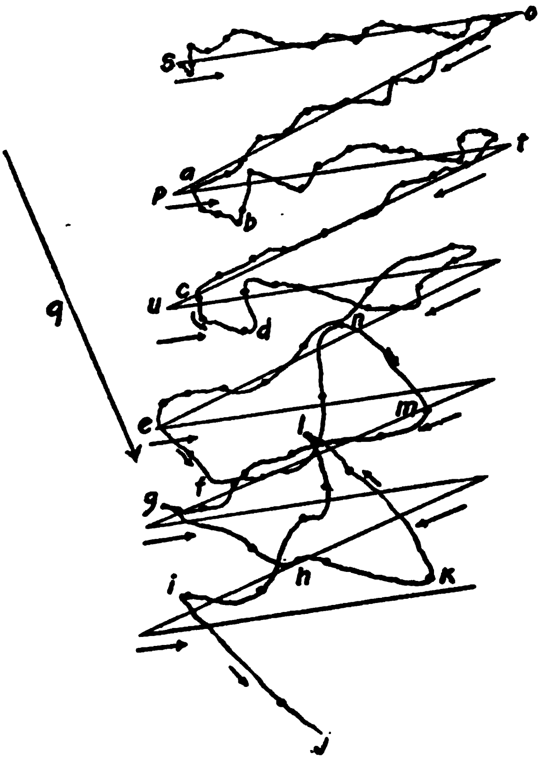


Fig. 16. An original tracing of a geometrical diagram by Subject No. 3. The tracing began at *s* and ended at *j*. The subject was asked to begin at *s* and trace the diagram as indicated by the arrows, gradually increasing his speed as he proceeded. The stations on the tracing line mark the time in fifths of a second. The control was lost in most instances on the line moving from left to right; the apparent direction of this line is indicated by the large arrow, *q*. Analysis of the tracing reveals the fact that the lines *ab, cd, ef, gh, ij, kl,* and *mn* are all parallel to the apparent direction of the line to be traced.

curve, a subject working under recess period conditions, with speed and accuracy instructions, was compelled to choose between accuracy and speed,—he could not secure both, Fig. 6, p. 16. Referring to our findings on the initial delay, we see that the recess period increases the initial delay if accuracy is secured or, in other words, if the muscular synthesis required for the accuracy is maintained. Thus, if we consider the tracing of the whole star as made up of a number of difficulties,— each involving a visual fixation of the path, a period of delay, and then a movement,—we can see that the recess period will bring about a number of these delays which will work to increase the time of the tracing, our irradiation curve. On the other hand, series practice will eliminate these periods of delay to some extent and thus work to produce our speed of facilitation, Fig. 14. From this it may be seen that both speed and accuracy may be secured in series practice, provided the initial drop in the learning curve has been passed, Fig. 5, p.—.

It should be noted that a low rate of facilitation is obtained on continued repetition of a tracing under recess period practice conditions, Figs. 2 and 14, parts I and IV of the latter. Thus we have two types of facilitation, one through repetition in series and the other through prolonged repetition under recess period conditions. The physiological structure underlying both types is precisely the same. (See our discussion on the adequate recess).

In the later stages of the one-stroke practice, Fig. 15, it will be observed that this highly practiced subject reaches a point where the initial delay before each successive stroke becomes constant, the strokes have become rhythmical and the subject is securing just enough delay before each stroke to insure the perseveration of the irradiation pattern in the musculature of the tracing arm. Marked individual differences obtain with respect to this initial delay and rhythm of stroke among different subjects; a point that we are utilizing in a further study on the economy of learning. It is worthy of special note that this rhythm is, with some subjects, maintained over short intervals, which gives the reaction time a degree of constancy not found in earlier stages of the learning, and thus brings about a state of affairs where the short recess period has little or no effect on the initial delay. However, an unusually long recess will reestablish the long initial delay as shown in Fig. 17.[26] This state of affairs is usually called forgetting in such performances as typewriting or crossing out letters from pied type. (See discussions by Thorndike (49) where the functions of the recess are conceived solely as bringing about forgetting and eliminating fatigue).

---

[26] It should be noted that tracings of the whole star by the highly practiced subject show less effect from the action of the recess period than do tracings of one side as shown in Fig. 15. This is due to the fact that the facilitation in the former is much less than in the latter. In series practice on the whole star each difficult element is repeated every ten seconds, on the average; while, in the one-stroke practice, the stroke is repeated ten times as fast.

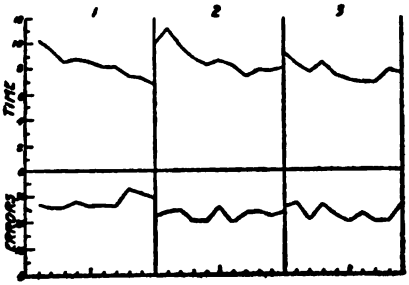


Fig. 17. This drawing presents the record of Subject No. 4 upon apparatus I after he had acquired a high rate of speed. Here are shown three groups of circuits in series; a one-week recess came between groups. It should be noted that the recess period has its greatest effect upon the speed of the tracings, due to the initial delay required in difficult places.

f. *Relation between Instructions and Type of Practice.*—Recess period practice makes possible the highest type of accuracy, and mixed series and recess period practice makes possible the highest degree of speed. A highly trained subject will have his efficiency greatly reduced if speed instructions are combined with recess period practice; or, if accuracy instrutions are combined with series practice: the highest efficiency is always secured by combining recess period practice with accuracy instructions, and series practice with speed and accuracy instructions. This state of affairs, which is another aspect of the problem we have just discussed under the initial delay, is clearly shown by Table I.

B. Physiology of the Recess Period

a. *Character of the First Movements.*—The very first movement employed by the beginner in learning to trace the star is invariably a movement in the apparent direction of the path to be traced, *AB,* Fig. 18, or, in other words, it is in the direction of the path *AC.* This first movement leads to a contact with the side of the path at *d;* at this point the stylus usually becomes

Circuit	Speed and Accuracy Instructions						Accuracy Instructions			
	Recess Period Practice				Series Pr.		Recess Pr.		Series Pr.	
	I		II		III		IV		V	
	T	E	T	E	T	E	T	E	T	E
1	15	22	21	15	22	18	30	1	30	2
2	15	20	20	10	15	7	27	2	33	4
3	15	21	19	10	21	19	32	0	35	0
4	15	12	18	12	17	16	32	1	32	0
5	12	28	22	14	20	15	32	0	30	3
6	12	20	21	15	14	12	32	0	33	3
7	13	17	20	5	16	13	31	2	30	3
8	14	19	15	15	15	10	35	1	35	1
9	13	16	15	17	15	12	30	2	30	0
10	13	16	15	15	14	12	35	1	25	2
Av.	13.7	19.1	18.6	12.8	16.9	13.4	31.6	1.0	31.3	1.8

TABLE I. In this table is shown data taken from the work of subject No. 21; the results were secured from one day's practice after the subject had acquired a high degree of skill. Under the speed and accuracy instructions, I emphasized speed and II emphasized accuracy, while in III the subject was left uninstructed except as to general speed and accuracy instructions. If we take an arithmetical average of the time and error quantities, we find that II is better than I, while III is better than either for speed and accuracy instructions. Under accuracy instructions, recess practice secures a much higher degree of accuracy than does series practice.

caught in a niche, and the subject attempts to withdraw it; these attempts in most cases serve to drive the stylus more deeply into the niche. After a number of these series attempts the stylus is withdrawn quickly in the direction of *de,* which movement is a reciprocal of the first movement, *ad.* (By a reciprocal we mean not a direct opposite of the movement *ad,* but an opposite modified by the common muscular element *AH.* It should be noted that the mirror produces no illusion with respect to tracings directly toward the right or the left; the illusion is confined entirely to those paths which run obliquely to the *AH* axis).

Following the movement *de,* we have in immediate succession the movement *ef,* which is a reciprocal of *de,* modified by the common muscular element *AH;* the movement *ef* is made up of the same muscular elements as *ad.* This series of movements serves to carry the stylus along the path to be traced. It should be observed that these are not only successive movements, but that any two successive movements involve many opposed muscular elements, that is, opposed in a physiological sense, disregarding, as before, the common muscular element *AH.*

While in the above account we have simplified to some extent

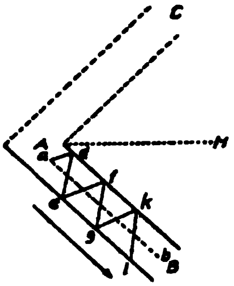


FIG. 18. A diagrammatic tracing of the first side of the star.

the type of reaction the average subject makes in his first attempts at tracing the path, yet, we have not in any sense falsified the true state of affairs. A subject may, at the outset, follow a very complicated course in tracing the first side of the star; yet, he will eventually reduce his behavior to as simple a pattern as the one we have indicated. It is also true that many subjects, even at the outset of the learning, do not produce a more complicated tracing than we have just described, Fig. 12, p. 49.

It is our finding that these successive movements which involve opposed muscular elements, are reduced by the action of the recess period, to one movement, with the muscular elements acting simultaneously. This simultaneity occurs, in the majority of cases, immediately after the recess interval; in other words, a zigzag course before the recess interval is resolved, through the interposition of the recess, into a simple, smooth course down the middle of the path.[27]

b. *Simultaneous Irradiation or Appropriation of Muscular Elements in Recess Period Practice.*—The movement down the middle of the path can be readily explained by the principle of the simultaneous functioning of neuromuscular elements which, before the recess interval, functioned successively. This simultaneous functioning has for its neurological basis a simultaneous irradiation, which is brought about by the action of the recess interval. Here we see that the number of neuromuscular elements from the cortical center to the arm, that act simultaneously, has been increased by the effect of the recess period. It should be noted that we are presenting here a neurological conception which may be classed as a ***multiple final path*** conception, in contradistinction to the principle of the ***final common path*** of Sherrington, where many afferent fibres embouch upon a few efferent fibres, (40).[28] It is also worth noting that Pawlow (28, 55) has accounted for his conditioned reflex by the principle of irradiation; however, it does not seem that Pawlow has emphasized the simultaneous character of this irradiation.[29] This simultaneous irradiation, brought about by the recess period, is the physiological basis for one phase of neuromuscular coordination; and is, in our judgment, essential for a complete conception of the integrative functions of the nervous system, particularly, of the cerebrum. No one has seen this so clearly as Stiles in his study of the nervous system (43).[30] It is not maintained that one recess period will always bring about a complete simultaneity in the function of all of the muscular elements which have previously functioned successively; but, the recess period always works toward this simultaneity of function of opposed muscular elements.

---

[27] The reader should note that the first attempts made by the learner are in close succession, and thus are really in series. The learner brings to the situation a definite irradiation pattern—tracing by direct vision—which is not satisfactory for tracing by mirror reflection and must be disintegrated by series attempts.

c. *Elimination of Muscular Elements in Series Practice.*—The

---

[28] That Sherrington was in need of a physiological principle underlying the simultaneous functioning of opposed muscular elements, is clearly seen from his statement that the effect of strychnine in the spinal dog, which produces a simultaneity of function of opposed muscles, helps us to see how the same simultaneity might occur in voluntary movements, (op. cit. p. 106 and p. 113).

[29] This, he would not be expected to do, unless he were contrasting the simultaneous with successive irradiation, which he nowhere does. Our emphasis upon the function of the recess period entails a further refinement of terms. Pawlow, incidentally points out the necessity for time intervals between the presentations of stimuli in the establishment of the conditioned reflex; but, he does not, to the writer's knowledge, establish the recess period as a definite physiological principle underlying the irradiation.

[30] Stiles points out (p. 94) that in slow, highly controlled movements a large number of neuromuscular elements is functioning; while, in quick, rapid movements, but a few units are in function. Here, Stiles does not attempt to deal with the problem of irradiation; he is interested wholly in the number of neuromuscular elements entering into movements of various types.

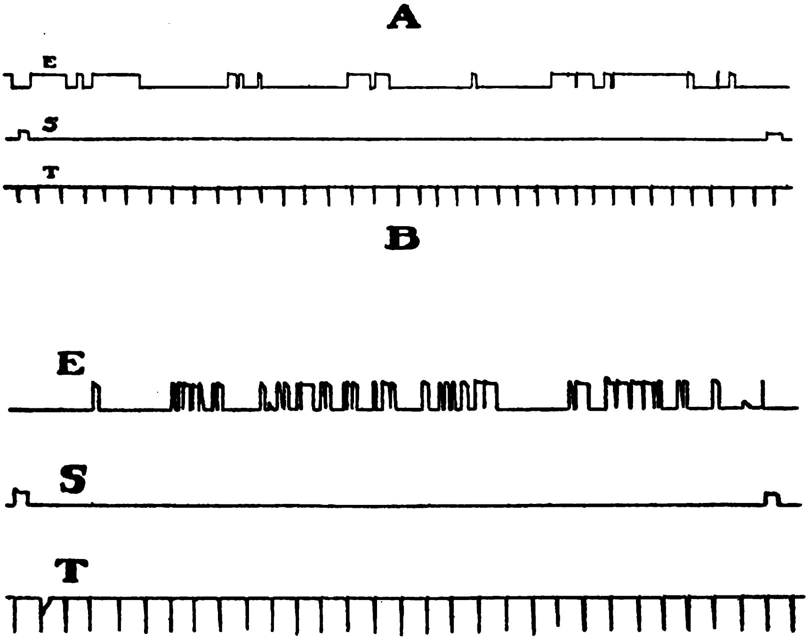


FIG. 19. An exact reproduction of two original records from the recording instrument of apparatus I. Circuit *A* was preceded by a recess interval, while *B* followed without recess. In *A* the instructions were accuracy, and in *B* speed and accuracy. Note that in *B* more errors are made; the errors are made in pairs, indicating rapid movements from one side of the path to the other and pointing clearly to the restoration of the zigzag path. The *E* line represents errors, *S* the signal for starting and stopping, and *T* the time in seconds. (Allowance should be made for the variation in the speed of the drum in the two cases).

function of series practice is to reduce the size of the irradiation pattern which has been built up by recess period practice. This reduction of the neuromuscular elements engaged in the tracing leads to marked facilitation involving rapid and easy movements. If the reduction of the irradiation pattern takes place gradually, the bilateral character of the pattern will be preserved and the subject will maintain his control. If, however, the subject be placed under speed instructions and the accuracy element disappear from his goal idea, the bilateral irradiation pattern will be completely disintegrated, Figs. 15, 16 and 19.

In the latter case, we are dealing with the problem of successive irradiation rather than with simultaneous irradiation, i.e., first one group of muscles receives the nervous impulse and then another. The neural energy arising in the retina is carried by the afferent nerves to the cortical center, and there passes out over first one efferent nerve fibre, and then another; thus there is a *successive* use of the opposed efferent paths,—a state of affairs in harmony with Sherrington's principles of reciprocal innervation.[31] The same loss of control, that is, a reduction of a simultaneous functioning of muscular elements to a successive or solitary functioning, also occurs when the subject is put under speed instructions and asked to trace the diagram of Fig. 9, p. 41. The first stroke that a trained subject makes when asked to make one rapid stroke down the side *AC* of this figure, is in the apparent direction, *AB,* of the path; that is, he employs again the first stroke he attempted to use when he began mirror tracing. In other words, the synthetic neuromuscular complex *AC,* acquired in the learning through the effect of the recess, has been completely eliminated as a result of the instructions, and we have left simply the *AB* element.

We modified our experiment so as to throw light upon the effect of alcohol upon the coordinations in a highly trained subject. From these experiments, Fig. 20, we are able to draw the significant fact that the alcohol tends to transform the simultaneous functioning of the opposed muscular elements into a successive functioning of these elements. In other words, the subject who has acquired the smooth course of the stylus down the middle of the path is, through the effect of alcohol, reduced to a stage where the zigzag path returns and his accuracy or control is lost. Our experiment shows that there is a slight loss in speed and a marked loss in accuracy, due to the effect of the alcohol; the total effect of the alcohol is the same as results from speed instructions.

---

[31] It is to be borne in mind that Sherrington worked chiefly on the spinal dog and, as a result, his principle of the final common path and reciprocal innervation applies mainly to the spinal centers; however, he does not apply this principle to cerebral functioning. In fact, one gathers from a careful study of Sherrington that he considers this principle of the final common path one of the fundamental principles of nerve integration—a fact not in harmony with our findings.

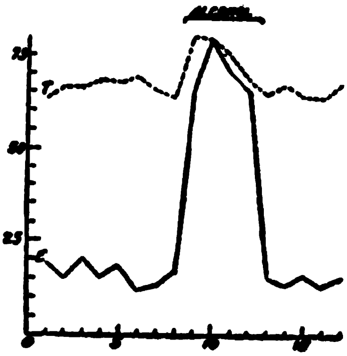


FIG. 20. An experiment showing the effect of alcohol, performed upon Subject No. 18. This subject had reached a level of efficiency which varied only slightly from day to day. Each station in the curves represents an average of five circuits in series; the series were separated by recess intervals of five minutes. In a previous experiment a much smaller amount of alcohol was used and the results indicated a small loss in accuracy and a slight gain in speed.

d. *Later Stages of Irradiation.*—By referring to Fig. 15, it will be observed that the irradiation produced by the recess period becomes less and less as series practice is inserted into the practice; which means that, in time, very little irradiation will be shown in the learning curve. This irradiation may be restored by the insertion of a very long recess interval, as in Fig. 17, where a high rate of speed obtains.

Inasmuch as, in the earlier stages of the learning, we get such a high rate of disintegration from series practice, Fig. 2, p. 13, and such a marked integration from recess period practice, it might be assumed that the subject acquires an immunity to these forces of integration and disintegration as practice continues. This is true simply because the learner faces a different situation in the two cases. In the early stages of practice the learner does not have the appropriate movement and, as a result, he fixates visually the same point on the star many times in close succession. We have the record of one subject who repeated the same movement (tracing in the apparent direction) forty-seven times in tracing the first side of the star. In later stages of practice, when the subject has acquired the mirror tracing coordination, the same point on the star is visually fixated only once in every ten seconds, on the average.

This state of affairs is also clearly shown when we take any highly practiced learner and place him in our one-fast-stroke practice, Fig. 15. His rate of neural disintegration or loss of control will be seen to depend upon three factors: (1) an individual difference factor, dependent upon bodily metabolism, (2) state of health, which shows considerable variation from day to day, (3) temporal frequency of the strokes, in other words, the speed of stroke and the initial delay allowed by the conditions of practice. If allowed to select his own conditions each learner will adopt a rhythm suited to his own rate of anabolism and katobolism.

e. *The Adequate Recess Interval.*—While we have found throughout our experiment that the recess interval is the sole factor which brings about the necessary simultaneity in neuromuscular functioning; yet, it has been extremely difficult to measure, on all occasions, the precise length of interval required for such a physiological state. This is due to the fact that individual differences obtain here in a very marked degree; some subjects may acquire the simultaneous irridation necessary for the early stage of the learning, through the insertion of rather short recess intervals; yet, others may require, for adequate irradiation, time intervals which are considerably longer and more frequent. We have found, for example, in our experimentation, that some subjects will have the zigzag path resolved into a smooth path by recess intervals as short as ten seconds, while others evidently require recess intervals which are considerably longer. We have not observed a case of simultaneous irradiation in the very early stage of learning after a recess interval shorter than ten seconds.

There is much evidence that the adequate recess interval varies greatly from time to time in the same individual, depending, perhaps, upon his general metabolic condition. From our experiments involving recess periods of a few seconds and others of varying lengths up to one year, we are able to derive the law that the major part of the structural changes underlying the simultaneous irradiation,[32] brought about by the recess interval, takes place

---

[32] That these changes are metabolic and have to do with the anabolic processes of restoring the cerebral cell to the condition existing before stimula-

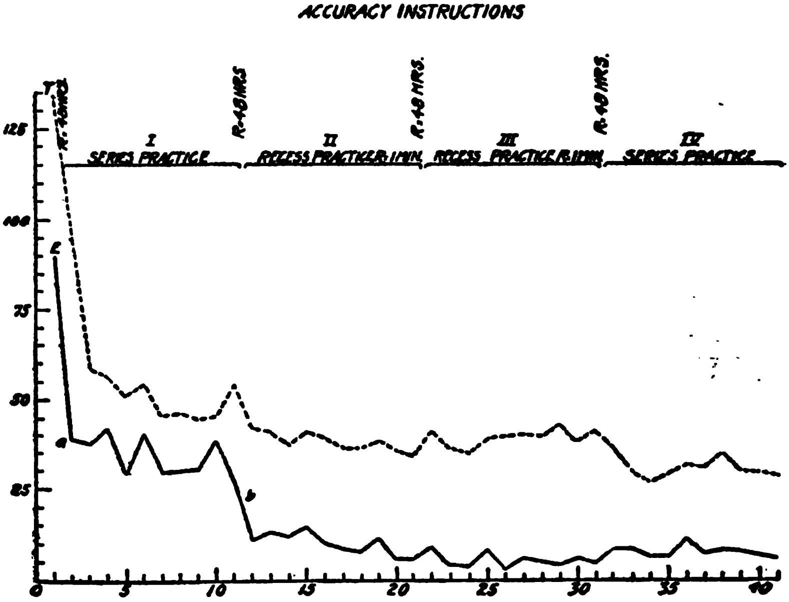


FIG. 21. A curve made from the average of three subjects who show a gain in accuracy in early series practice. A comparison between this curve and that in Fig. 2 reveals the fact that the subjects in the latter group were affected more by the recess periods than the former. These three subjects were the only ones in a class of thirty students to show improvement in accuracy in early series practice. See Table II, *B*.

in the earlier part of the interval. There are some evidences to indicate that the curve for these changes, secured through average results of a group, will parallel closely that found by Ebbinghaus (11) in his curve of forgetting, and in particular the curve found by Finkenbinder (12) in his analysis of the process of forgetting.[33]

---

tion, we have no doubt. Matthews has pointed out (Physiological Chemistry, Wm. Wood Co., 1916, Chap. XIII) that in the brain, we have all the necessary provisions made for the most intense and most rapid metabolism to be found in the entire body. Our results show that some subjects, reduced to a condition in the early stages of practice where improvement is not possible, will recover within ten seconds.

[33] At the present time an investigation is in progress at the University of Utah which aims to determine the exact length of recess interval required for different learners at various stages of the learning. In this investigation we are employing the rate of assimilation of muscular elements, brought about

There are numerous evidences to indicate that the effect of the recess period is relative and depends upon the type of practice the learner has undergone. Thus, if the learner has been making one circuit of the star per day for twenty-five days, a recess of a week, a month, or even nine months, (Fig. 7), will not noticeably reduce the time of the tracing. However, if a subject has been making ten circuits in series per day, and has thus acquired a high rate of speed, the speed of tracing will be very much reduced by the insertion of a week's recess in his practice, (Fig. 17). It would seem in the first case that the cortical irradiation has reached a stable form or pattern, due to the twenty-four hour recess periods between practices, and probably does not enlarge appreciably during a recess; while, in the case of an irradiation pattern built up by series practice, we have a small pattern which would be greatly enlarged by the insertion of a long recess interval, and thus the speed of the tracing is reduced.

The pattern of bilateral irradiation seems to be stable under conditions of one circuit per day practice, which is shown by the fact that an arithmetical average of the time and error quantities will be approximately constant, (later stages of the learning in Figs. 4 and 6). Our quantitative results show that if circuits are multiplied oftener than once per day the above constant does not occur, and the improvement continues to take place, at a logarithmic rate, throughout practice, Figs. 5 and 7. For purposes of clearness, we have called all improvement in time of tracing, taking place from the reduction of an irradiation curve by means of practice with twenty-four hour recess periods, Fig. 3, Part I, *primary facilitation;* while that gain in speed of tracing brought about by series practice, or with recess periods of less than twenty-four hours, is *secondary facilitation.* Primary facilitation is thus seen to be limited by the speed of functioning of a stable irradiation pattern, built up by twenty-four recess periods; secondary facilitation is unlimited, except physiologically.

We have been frequently amazed at the entire absence of

---

by recess periods of varying length, and the rate of dissimilation, brought about by series practice. While the problem presents numerous difficulties, it does not seem impossible of solution.

forgetting in this problem. In a number of instances, (Figs. 4, 5 and 7), recess intervals as long as eight months in length occurred in the learning without any visible sign of forgetting as a result. The explanation for this seems, to the writer, to lie in the fact that the coordinations acquired in this act of learning are related to the elements of a situation—mirrorwise perception—not found in the case of other acquired coordinations. If the subject lived in a mirrorwise world, there would then be forgetting in mirror tracing, since the coordinations acquired for similar situations would become fused and thus produce a definite loss through interference,—the chief factor in motor forgetting. It is worthy of note that the absence of forgetting in our problem has contributed much to our findings relative to the positive value of the recess period.

f. *Behavior of the Eye under Various Conditions.*—We have already indicated in our introspective analysis that during recess period practice under accuracy instructions, the visual attention of the subject is directed to the details of the path to be traced by the stylus; this visual attention assumes the character of fixation. In series practice, or in practice under speed instructions, the visual attention of the subject was found to lose this fixation character and to become distributed over a larger and larger area of the star as practice proceeds. In this case the details of the path to be traced are not observed; the subject reacts to his visual impression of the general direction of the path.

It is now only necessary to observe that when the visual attention is narrowed down to details, during recess period practice, the neuromuscular elements from the cortical center to the arm, are functioning simultaneously; but when the visual attention is too much spread out, so as to perceive only the general direction of the path, the neuromuscular elements function, not simultaneously, but successively, or in solitary fashion. From this, we gather that the control in this problem is correlated with the behavior of the eye; and the analytical or obtuse receptor functions of the eye depend upon the conditions of practice,—series or recess period. Woodworth (54) found much the same state of affairs in his study of the accuracy of voluntary movements; although he does not carry the analysis so far as we have attempted in this investigation.

It is well to note at this point that our subjects in the alcohol experiment reported that the drug seemed to affect the eye in a very noticeable way, and, as a result, they were not able to visually fixate the details of the path. The tracings secured under the effect of alcohol show a definite loss in accuracy.

From the foregoing findings it is apparent that when the eye acts as an analytical receptor,—visual attention directed to details of the path,—we have simultaneous irradiation over the efferent neuromuscular paths; but when it acts as an obtuse receptor,—as in the perception of the general trend of the path,—we have successive irradiation, describable in terms of reciprocal innervation.

g. *The Work of Pawlow, Sherrington, and Others, in Relation to our Findings.*—Pawlow has explained the conditioned reflex by the principle of irradiation (29). He has also observed (32) that this irradiation or conditioning takes place only when the stimuli are separated by time intervals. From this it seems that the Pawlow conception of the conditioned reflex and our own findings as to the physiological foundations of the learning in this investigation are in perfect harmony.

A somewhat clearer insight into the relation between Pawlow's work and our own findings, however, is secured by a further analysis of his experiments. He succeeds in conditioning a new stimulus when that stimulus is applied intermittently, but simultaneously with or in close succession to the functioning of the unconditioned response. Thus, the meat-powder stimulus applied to the tongue is simultaneous with the ringing of the bell; and, after a number of repetitions of this combination, the sound of the bell is observed to stimulate the secretion of the saliva, which was formerly brought about only by the unconditioned, meat stimulus. There is then an irradiation from the auditory, affervent nerves to the salivary, efferent nerve fibres.

The relation of this principle of irradiation to our own problem will probably be yet more apparent, if it is assumed that the auditory stimulus originally produced an *unconditioned* response, such as the perking of the ears of the dog. Then it will be seen that, after the conditioning, the auditory stimulus flows out over, not only the effervent paths which produced the perking of the ears, but also the salivary efferent paths which were in simultaneous function with the perking of the ears.

Pawlow and his pupils have also shown (30, 38, 52) that the conditioned reflexes are worn out by repetition without adequate recess intervals. When these conditioned reflexes are not completely worn out, they are partly restored by the action of the recess period. These investigations have also shown that the conditioning of an unnatural stimulus is always brought about by what they call an "analytical receptor"; in other words, the irradiation is produced by such a receptor.

The keystone in the arch of Sherrington's system is the final common path, which is, neurologically, just the opposite of our principle of the multiple final path in simultaneous irradiation. Sherrington's principle of the final common path has been deduced largely from his work upon the spinal dog, where his experiments have involved only the obtuse receptors below the medulla; thus, as it appears to us, his principle of the final common path is directly dependent upon the type of reaction secured through stimulation of the *obtuse* touch, or pain, receptors on the surface of the dog's body. The work of Sherrington needs, in our judgment, to be supplemented by the work of Pawlow, if we would have a physiology of the nervous system upon which human learning, even of the trial and error type, may rest. The work of Pawlow may be clarified by the findings of this investigation,—that one aspect of the learning, simultaneous irradiation, is brought about directly by the effect of the recess period which produces the analytical function of the analytical receptors. The work of Pawlow in pointing out the function of the analytical receptors, the foundation of the irradiation, is then seen to be supplemented by our finding that the eye may function as an analytical, or as an obtuse, receptor; and the manner of presenting the stimulus, in series or intermittently, determines the character of this function.

Our contention is that Sherrington's final common path conception is fairly adequate as an explanatory principle for certain nervous integrations of the spinal dog, where the irradiation is largely to closely allied musculature—immediate spinal induction in Sherrington's nomenclature; but it is far from adequate for the human subject or, even for the dog, with analytical receptors and cerebrum intact.

Our criticism of Sherrington's physiological system, that it is wholly inadequate to furnish a physiological foundation for human learning, leads us to a similar criticism of the system employed by Thorndike (49, 50) in his psychology of learning. Thorndike advances a semi-behavioristic system with but one physiological element,—repetition of the stimulus. He attempts to build a system about this one element by proposing his conception of "bonds" or "connections," functional terms indicating a state of affairs that requires some dynamic agency to bring about their existence; repetition alone cannot establish his connections. The dynamic element needed for this system is found in the "feeling of satisfyingness," which has, as its physiological foundation, the conduction of neurones ready to conduct,—a principle as vague as it is unphysiological. Without further necessary details regarding the structure of this "readiness of neurones to conduct," physiologists must certainly regard this state of affairs as interpretative and not a scientific description of an observable situation. Thorndike's predicament is not improved, for physiology, by invoking the aid of the psychological aspect of this readiness of the neurones to conduct, viz., the "feeling of satisfyingness," which occurs when neurones ready to conduct, do conduct, (50).

In our opinion the term bond, or connection, is an artifact, wholly unsupported by analysis, which is made to appear workable by a vague, as well as an unwarranted, assumption that the bond or connection is established, or "stamped in," by the feeling of satisfyingness following upon a chance establishment of the bond. Either the readiness to conduct or the satisfyingness is without physiological foundation. Science, clearly, must require an element, other than the repetition of a stimulus, in a physiological system that would explain human learning.[34]

Book, in his study of typewriting (4) points out that the purposeful use of a new habit seemed to be more of a hindrance than a help. He also holds (p. 171) that in later stages of the learning, the writing becomes harder and harder to attend to. This is in agreement with our contention that as practice proceeds, the visual attention spreads over a larger and larger field, and thus becomes more general; the details drop out.

---

[34] We are in agreement with Titchener (51) that, in the last analysis, psychological processes are explained by the underlying physiological processes.

Book holds that improvement after recess is due to the fact that during the recess period certain 'psychophysical difficulties' and 'interfering tendencies' drop out. On further analysis one finds that the interfering tendencies which he has in mind are made up, at least in part, of the ever present tendency of the subject to attempt speed before the co-ordinations are ready. In support of this fact, Book holds (p. 179) that the vital problem in learning is determining just how fast to push ahead. Now, it is the recess period, as has been shown in this investigation, which corrects this tendency to push ahead too fast and brings the individual back to a stage when his attention is devoted to the details. Book points out also (p. 179) that the learner can do nothing to control his fluctuations of attention; "it is not what the learner would like to do, but what his mental and physical conditions will let him do." This is in substantial agreement with the results obtained in this investigation.

We find also that our findings are supported by many of Woodworth's results in his study of the accuracy of voluntary movement (54). Throughout his study he contends that visual control works for accuracy unless the movement be at high speed; "at high speeds the accuracy contributed by voluntary attention, using either eyes or muscular sense, amounts to zero; by decreasing the speed we greatly increase the accuracy due to visual control." This is in agreement with the position taken in this investigation on the relation of visual control to speed.

Woodworth points out (p. 39) that "if we distinguish between initial adjustment and current control, we see that the conditions of current control are not changed by altering the time interval between strokes; a long interval works slowly on the initial adjustment." He finds, further, (p. 74) that when the speed of a movement, as in handwriting, is slow enough 'to permit fine secondary adjustments' the eyes assist greatly in securing accuracy; when the eyes are not used, however, he finds that "extreme slowness is a disadvantage; higher speed of writing movement is secured with eyes closed than with eyes open." This is in complete agreement with the findings in this investigation that the visual perception when concentrated leads to accuracy.

Many other investigators, such as Binet (2), Pechstein (33) and Lashley (24) have found experimental confirmation of the value of the recess period. Finally, we should note that James (18) made famous the observation that "we learn to skate in summer and swim in winter," which, more than any other observation, has stimulated this investigation.

IV. SUMMARY

In this study of trial and error learning, we have made an analysis of the behavior of human subjects under definite instructions and before a constant situation,—namely the tracing of a simple diagram by mirror reflection. The study has revealed at every point the behavioristic nature of this type of learning; it has shown that learning to trace, by mirror reflection, a line which runs obliquely to the median plane of the body, consists ~~entirely in acquiring a~~ modification of the response the subject brings to the situation ~~as a beginner~~. A description of the progressive modification of this response, under such conditions as varied instructions to the learner, varied temporal distribution of practice periods, constitutes the material of this paper.

We have observed the repetition of the response of the learner with and without recess periods between the repetitions, and our findings here are to the effect that the recess period, in early stages of the ~~learning, makes possible the~~ attainment of accuracy; and that repetitions without recess, in later stages of the learning, make possible the attainment of speed. Accuracy was shown to depend upon the simultaneous functioning of certain opposed muscles—simultaneous irradiation—predominantly of the used arm; speed was seen to depend upon two closely related factors, —namely, elimination of unnecessary musculature and elimination of the period of delay required and made possible by recess period practice when the eye is fixated narrowly upon the details of the tracing; these two elements, in our interpretation, constitute physiological facilitation.

In accuracy tracings the eye was shown to act as an analytical receptor, being fixated upon the details of the tracing; in speed practice the visual perception is spread out over a large area of the objective situation. When the eye is acting as an analytical receptor, simultaneous irradiation obtains in the efferent neuromusculature of the body parts concerned in the tracing. When the eye begins to act as an obtuse receptor, the functioning efferent neuromusculature becomes reduced and, as a last stage of this, when great speed has been attained, the efferent neuromusculature functions according to the principle of reciprocal innervation, that is, only closely allied neuromuscular elements are innervated and the opposed elements are inhibited.

The value of instructions given to a learner was found to depend wholly upon the physiological conditions resulting from the type of practice—series or recess period—under which the learner is working; recess period practice making accuracy possible, and series practice, speed.

It was shown that the mental image of a movement in mirror tracing cannot control the movement. The feeling of pleasantness following a satisfactory movement was shown to have no effect upon the reappearance of the movement; and this feeling of pleasantness was shown to have no place as an element in bringing about the improvement at any stage of the learning.

BIBLIOGRAPHY

1. Bergstrom, J. A. Effect of changes in time variables in memorizing. Amer. Jour. of Psych., 1907, Vol. 18, pp. 206-238.
2. Binet, Alfred. La fatigue intellectuelle. Paris, Librairie C. Reinwald, Schleicher Freres, 1898, 336 p.
3. Bok, S. T. Die Entwicklung der Hirnnerven und ihrer Zentralen Bahnen. Die Stimulogene Fibrillation. Folia neuro-biologica, 1915, Vol. 60, pp. 475-565.
4. Book, W. F. The psychology of skill with special reference to its acquisition in typewriting. University of Montana Pubs. in Psychology, 1908, No. 1, 188 p.
5. Brown, T. G. On the phenomenon of facilitation. Quart. Jour. of Exp. Physiology, 1915, Vol. 9, 81-99.
6. Burnham, W. H. The significance of stimulation in the development of the nervous system. Amer. Jour. of Psych., 1917, Vol. 28, pp. 38-56.
7. Cameron, E. H. and Steele, W. M. The Poggendorf illusion. Psych. Rev. Mono. Suppl., 1905, Vol. 7, No. 29, pp. 83-111.
8. Carr, H. Apparent control of the visual field. Psychol. Review, 1907, Vol. 14, pp. 357-382.
9. ————. Voluntary control of the distance location of the visual field. Psychol. Review, 1908, Vol. 15, pp. 139-149.
10. Dodge, R. An experimental study of visual fixation. Psychol. Review Mono. Suppl., 1907, Vol. 8, No. 35, 95 p.
11. Ebbinghaus, Hermann. Memory (Trans. by H. A. Ruger). Teachers College, Columbia University, New York, 1913, 123 p.
12. Finkenbinder, E. O. Curve of Forgetting. Amer. Jour. of Psychol., 1913, Vol. 24, pp. 8-32.
13. Flügel, J. C. Some observations on local fatigue in illusions of reversible perspective. British Jour. of Psychol., 1913, Vol. 6, pp. 60-77.

14. Freeman, F. N. Preliminary experiments on writing reactions. Psychol. Review Mono. Suppl., 1907, Vol. 8, pp. 301-333.

15. Galton, Francis. Inquiries into human faculty. London, 1883, 387 p; and Everyman's Library, 261 p.

16. Hicks, V. C. and Carr, H. A. Human reactions in a maze. Jour. of Animal Behavior, 1912, pp. 98-125.

17. Hylan, J. P. and Kraepelin, E. Ueber die Wirkung kurzer Arbeitzeiten. Psychologische Arbeiten, 1902, Vol. 4, pp. 454-494.

18. James, William. Principles of psychology. New York, Henry Holt, 1890. Vol. 1, 689 p.; Vol. 2, 689 p.

19. Jennings, H. S. Contributions to the study of the behavior of lower organisms. Publications of the Carnegie Institution of Washington, 1904, 256 p.

20. Kappers, A. Phenomena of neurobiotaxis in the central nervous system. Folia Neuro-Biologica, 1913, Vol. 7, pp. 55-56.

21. ————. Ueber das Rindenproblem und die Tendenz innerer Hirnteile sich durch Oberflachen Vermehrung, statt Volumzunahme zu vergrosseren. Folia Neura-Biologica, 1914, Vol. 8, pp. 507-531.

22. Ladd, G. T. and Woodworth, R. S. Elements of physiological psychology. New York, Scribners, 1911, 704 p.

23. Langfeld, H. S. Voluntary movement under positive and negative instructions. Psychol. Review, 1913, Vol. 20, pp. 459-478.

24. Lashley. K. S. A causal factor in the relation of the distribution of practice to the rate of learning. Jour. of Animal Behavior, 1917, Vol. 7, pp. 139-142.

25. Lucas, Keith and Adrian, E. D. On the summation of disturbances in nerve and muscle. Jour. of Physiology, 1912, Vol. 44, pp. 68-125.

26. Morgan, C. Lloyd. Animal behavior. London, 1900.

27. Morgulius, Sergius. The auditory reactions of the dog studied by the Pawlow method. Jour. of Animal Behavior, 1914, Vol. 4, pp. 142-145.

28. ————. Pawlow's theory of the function of the

central nervous system and a digest of some of the more recent contributions to the subject from Pawlow's laboratory. Jour. of Animal Behavior, 1914, Vol. 4, pp. 362-379.

29. Nicolai, G. F. Die physiologische Methodik zur Erforschung der Tierpsyche, ihre Möglichkeit und ihre Anwendung. Jour. f. Psychol. und Neurol., Bd., 10, 1907, pp. 1-27.

30. Orbeli, L. A. Conditioned reflexes resulting from optical stimulation of the dog. Dissertation, St. Petersburg, 1908, 111 p.

31. Pawlow, J. P. Sur la secretion psychique des glandes salivaires (Phenomenes nerveux complexes dans le travail des glandes salivaires). Arch. Intern. de physiol., 1904, T. 1, pp. 119-135.

32. ——————. The scientific investigation of the psychical faculties or processes in the higher animals. London, Lancet, 1906, pp. 911-915; also Science, N. S., 1906, Vol. 24, pp. 613-619. (Both are abstracts).

33. Pechstein, L. A. Whole vs. part methods in motor learning. Psychol. Review, Mono. Suppl., 1917, Vol. 23, No. 2, 80 p.

34. Perrin, F. A. C. An experimental and introspective study of the human learning process in a maze. Psychol. Review Mono. Suppl., Vol. 16, No. 4, 104 p.

35. Peterson, Joseph. Completeness of response as an explanation principle in learning. Psychol. Review, 1916, Vol. 23, pp. 153-162.

36. ——————. The effect of length of blind alleys on maze learning; an experiment on twenty-four white rats. Behavior Monog., 1917, Vol. 3, No. 4, 53 p.

37. Ruger, H. A. The psychology of efficiency; an experimental study of the process involved in the learning of mechanical puzzles and in the acquisition of skill in their manipulation. Arch. of Psychol., 1910, II, 88 p.

38. Selionyi, G. P. Contribution to the study of the reactions of the dog to auditory stimuli. Dissertation, St. Petersburg, 1907, pp. 125.

39. Sherrington, C. S. Nervous rhythm arising from rivalry of antagonistic reflexes; reflex stepping as outcome of double reciprocal innervation. Proc. Roy. Soc. B, 1913, Vol. 86, pp. 233-261.

40. ——————. The integrative action of the nervous system. New Haven, Yale University Press, 1906, 402 p.

41. ——————. Reflex inhibition as a factor in the co-ordination of movements and postures. Quar. Jour. Exper. Physiol., 1913, pp. 251-308.

42. Starch, D. A demonstration of the trial and error method in learning. Psychol. Bull., 1910, Vol. 7, pp. 20-23.

43. Stiles, P. G. The nervous system and its conservation. Philadelphia, Saunders, 1914, 229 p.

44. Swift, E. J. Relearning a skillful act; an experimental study in neuro-muscular memory. Psychol. Bull., 1910, Vol. 7, pp. 17-19.

45. Tashiro, S. Physiology; on the nature of the nerve impulse. Proc. Nat. Acad. Science, 1915, Vol. 1, pp. 110-114.

46. Thorndike, E. L. Animal Intelligence. Psychol. Review Mono. Suppl., 1898, No. 4.

47. ——————. The mental life of the monkeys. Psychol. Review Mono. Suppl., 1901, No. 5.

48. ——————. Ideo-motor action. Psychol. Review, 1913, Vol. 20, pp. 91-106.

49. ——————. Educational psychology; Vol. II of the psychology of learning. New York, Teachers College Press, 1913, 452 p.

50. ——————. Educational psychology; briefer course. New York, Teachers College Press, 1914, 430 p.

51. Titchener, E. B. Text Book of Psychology. New York, Macmillan, 1911, 552 p.

52. Usiewitch, M. A physiological investigation of the auditory capacity of the dog. Bulletin of St. Petersburg Military Medical Academy, 1911, Vol. 24, pp. 484-502.

53. Watson, J. B. Behavior, an introduction to comparative psychology, New York, Henry Holt, 1914, 439 p.

54. Woodworth, R. S. The accuracy of voluntary movement. Psychol. Review Mono. Suppl., 1899, No. 13, pp. 77-86.

55. Yerkes, R. M. and Morgulius, Sergius. The method of Pawlow in animal psychology. Psychol. Bull., 1909, Vol. 6, pp. 257-273.

TABLE II. (folded insert opposite). This table shows the quantitative data from the work of thirteen students, all working under identical conditions as to instructions to subjects and conditions of practice. The exact length of the recess intervals is shown in Figs. 2 and 21, which were plotted from the averages shown in *A* and *B* of the table. The headings at the top indicate the conditions of practice maintained thoroughout the experiment.

Part *A* of the table represents the work of ten students who did not show improvement in accuracy in the early part of the learning during ten circuits of no recess practice. Part *B* represents the work of three students who did show a slight improvement in accuracy during this practice; here, however, the improvement is so slight as to be almost negligible when compared with such improvement as takes place in early recess period practice, Figs. 4, 5, 6 and 7. It will be observed that the subjects in Part *B* are everywhere less affected by the recess periods than are the students in Part *A*.

Table I

				No Recess								
28	29	30	31	32	33	34	35	36	37	38	39	40
31	27	25	23	38	34	38	39	38	40	34	35	38
68	67	60	60	51	65	68	57	61	72	60	40	52
19	26	23	24	18	18	22	23	18	18	21	18	19
49	54	60	61	47	50	50	53	49	48	44	42	37
58	54	50	60	49	41	47	45	42	35	35	30	33
40	39	37	40	38	39	36	35	34	33	35	31	31
35	38	39	36	41	32	29	31	33	31	31	29	29
46	45	46	40	50	45	45	40	37	39	42	40	35
33	30	28	29	25	26	32	27	28	29	29	26	25
16	15	20	22	25	31	35	40	27	26	29	35	33
394	395	388	395	382	381	402	390	367	371	360	326	332
39.4	39.5	38.8	39.5	38.2	38.1	40.2	39.0	36.7	37.1	36.0	32.6	33.2
0	1	1	3	0	0	0	0	0	0	0	0	1
2	0	0	2	3	5	3	3	0	2	3	4	3
5	1	0	0	4	2	4	0	0	3	0	2	2
0	0	0	0	0	0	0	0	0	0	2	1	0
2	3	2	0	7	5	5	11	13	4	5	6	14
2	7	6	6	14	7	6	9	8	5	7	5	9
1	3	6	1	7	6	5	10	8	8	6	1	4
0	0	2	0	4	0	0	0	2	1	2	0	0
7	7	4	8	6	0	9	3	9	3	1	1	6
8	5	9	2	8	4	11	0	6	2	7	4	5
27	27	30	22	53	29	43	36	46	28	33	24	44
2.7	2.7	3.0	2.2	5.3	2.9	4.3	3.6	4.6	2.8	3.3	2.4	4.4
26	31	32	34	28	25	26	26	27	22	24	26	22
41	44	39	40	42	33	27	35	38	42	54	39	40
47	53	52	48	38	31	27	25	28	27	27	25	27
114	128	113	122	108	89	80	86	93	91	105	90	89
8.0	42.6	37.3	40.6	36.0	29.6	26.6	28.6	31.0	30.3	35.0	30.0	29.6
7	2	10	9	15	10	13	4	11	9	13	8	7
2	0	3	5	5	7	4	5	6	4	1	4	2
6	11	5	0	4	8	2	9	18	7	10	11	10
15	13	18	14	24	25	19	18	35	20	24	23	19
5.0	4.3	6.0	4.6	8.0	8.3	6.3	6.0	11.6	6.6	8.0	7.6	6.3

47

GENERAL LIBRARY
JUL 22
UNIV. OF MICH.

Vol. XXVIII
No. 3
PSYCHOLOGICAL REVIEW PUBLICATIONS
Whole No. 125
1920

THE
Psychological Monographs

EDITED BY
JAMES ROWLAND ANGELL, 522 FIFTH AVE., NEW YORK.
HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*J. of Exp. Psych.*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*) and
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

Work with Knowledge of Results versus Work without Knowledge of Results

Awareness and Partial Awareness as Factors Conditioning Efficiency

By
GEORGE F. ARPS
Professor of Psychology, Ohio State University

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.)
PARIS (16 rue de Condé)

Psychological Review Publications

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*J. of Exp. Psych.*)
JAMES R. ANGELL, UNIVERSITY OF CHICAGO (*Monographs*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*)
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews, notices of books and articles, psychological news and notes, university notices, and announcements, appears monthly, the annual volume comprising about 480 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 480 pages. (Suspended temporarily.)

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in May, and may be subscribed for in connection with the periodicals above, or purchased separately.

ANNUAL SUBSCRIPTION RATES

Review and Bulletin: $6. (foreign, $6.50). Journal: $3.25 (foreign, $3.50).
Review and Journal: $6. (foreign, $6.50). Bulletin: $3.25 (foreign, $3.50).
Journal and Bulletin: $6. (foreign, $6.50).
Review, Bulletin and Journal: $9. (foreign, $9.75).
Index with any other two: $7. (foreign, $7.50).
Review, Bulletin, Journal and Index: $10. (foreign, $10.75).

Current Numbers: Review, 65c; Bulletin, 35c; Journal, 65c; Index, $1.25.

THE PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. THE MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages, with a uniform subscription price of $5.50. (Postal Union, $5.80.)

Philosophical Monographs: a series of treatises more philosophical in character.
Library of Genetic Science and Philosophy: a series of bound volumes.

Subscriptions, orders, and business communications may be sent direct to the

PSYCHOLOGICAL REVIEW COMPANY

PRINCETON, NEW JERSEY

FOREIGN AGENTS: G. E. Stechert & Co., London (2 Star Yard, Cary St., W. C.);
Paris (16, rue de Condé)

INTRODUCTION AND STATEMENT OF PROBLEM

The data supporting this study were obtained by means of the Bergström Ergograph.[1] The well known unavoidable defects common to ergographic work are of immaterial value here except as they may exert a preponderating accidental influence in either the known or unknown series of experiments.[2] The long period of time over which the experiments extended would tend toward an equitable distribution of such defects and iron out any irregularity of emphasis to one or the other of the series.

The question of distribution applies to such defects as physiological variations and to mechanical variations of the machine such as leverage and adjustment. As to variations in the mental complex with reference to such features as attention and content there are wide differences according to whether the observer is fully or partially aware of his progress. The factor of tediousness must also be considered. However, the question of attentive fluctuation or tediousness, as it may relate to the degree of volitional innervation, may arise with reference to any work period of any series (known or unknown) of periods all conducted under constant conditions.

In the periods of a *known* series, i.e., where the observer is fully aware of his efficiency, attention and tediousness vary widely from the periods of an *unknown* series as is expected. In the former series, attention is heightened and tediousness lessened; in the latter series, especially in the initial work periods attention frequently lags and a certain ennui marked by a distinct feeling of depression often pervades the entire series. This is especially true of the periods in the series of the first set of experiments. In any series of work periods, the periods *inter se* proceed under varying degrees of attentiveness and interest. Since the efficiencies of work periods within a given series are not compared but rather the efficiencies of periods of different series, such attentive fluctuations can not for our purpose be considered defects. As far as variations in attention and tediousness apply to work periods of disparate series (i.e., known and unknown), we find in them conditions accounting for efficiency differences.

---

[1] This apparatus, which is not very simple, is described at length by Prof. Bergström in the Amer. Journ. of Psychology, Vol. XIV, 1903, pp. 510-540.

[2] T. Hough: Ergographic Studies in Neuro-Muscular Fatigue. Amer. Journ. of Psych., Vol. 5, 1901.

The question concerning which an experimental answer is here attempted is, whether a condition of relatively complete awareness of results is more or less favorable to efficiency than is a condition of partial awareness. To what extent, if any, does knowledge of results further efficiency; or, to what extent does a lack of knowledge curtail efficiency; or, is the aggregate accomplishment indifferent to differing degrees of conscious accompaniments. Again, is a response, in which a knowledge of results constitutes essential features, more or less efficient than a response when such knowledge is relatively lacking.

The work of Judd[3] on "Practice without Knowledge of Results" clearly permits the conclusion that greater efficiency results when the observer is aware of the conditions underlying the procedure of the experiment. It is probable that still greater efficiency would have resulted had the subjects observed by how much their reactions were in error.

Conduct of the Experiment and Attending Considerations

The data finally incorporated in this study were preceded by an unusually long period of preliminary practice. The entire period of experimentation, in the case of observer M, covers three years, two of which suffered certain unavoidable interruptions. The results of these two years are not included in this study and their chief value lies in the fact that practice effects reached a desirable minimum. The data, therefore, represent the efficiency values of trained muscles. In the cases of observers, J, and W, so extended a preliminary practice period does not obtain. All experiments were begun at approximately 9 A. M.

The hand and arm of the observers were strapped into the ergograph in the usual manner so as to secure maximum freedom for the middle finger of the right hand and to exclude, so far as possible, all irrelevant movements contributory to the efficiency of this finger.

---

[3] Psychological Rev., Monog. Supp., Vol. 11, 1905.

The study is divided into three sets of experiments; each set is divided into four series: ascending and descending known[4] and ascending and descending unknown. Each series of the first and second set is divided into work periods of eleven each. The known series consists of experiments in which the observer was fully aware of his results during the conduct of the experiment and of his results during previous known work periods. The records of the known periods were at all times open to the observer's critical examination; the records for the unknown periods were closed for the observer during, as well as after, the completion of an experiment. Both the known and unknown series consist of work periods made up of subdivisions each of which has a constant duration of ten seconds excepting the periods in which there are no rests. The rest between each of the subdivisions of the periods, common to the two series, varies from zero to ten seconds. Eleven work periods, each separated by an interval of forty-eight hours, constitute a series in which there is continuous work in the zero period; a rest of one second to every ten seconds of work in the first period; two seconds of rest to every ten seconds of work in the second period and so on until the seconds of rest equal the seconds of work, i.e., ten. It will be noticed that in the ascending known and unknown series of the first set and in the ascending unknown and known of the second set the number of rest seconds for each subdivision of the work periods of the series forms an arithmetical progression by a common difference of 'one' from zero rest in the zero work period to ten rests in the eleventh period. The corresponding decreasing series of the first and second sets form a descending arithmetical progression by the same difference with respect to the number of rests.

---

[4] Note: The terms 'ascending' and 'descending' apply to the order in which the work periods of a series are performed with respect to an increase (ascending) or decrease (descending) in the number of rests of successive periods.

The terms 'known' and 'unknown' will always refer to the observer's knowledge or lack of knowledge respectively of his efficiency.

The work in each period is continued to exhaustion, i.e., until the observer is no longer able to lift the ergographic load. The observers were instructed to make each effort, that is each lift, represent maximum voluntary contraction. Since the periods always continued to exhaustion it was desirable to extend the duration of the interval between periods—forty-eight hours—to permit of relatively complete restoration of the tissues and to avoid soreness. In each subdivision of the work periods the second failure to lift the load was the signal to stop the experiment. The muscle was then regarded as exhausted, and by this it should not be inferred that we hold it possible to extract the last item of movement by means of voluntary innervation.[5] The speed of lifting was fixed by a metronome beating sixty strokes to the minute. In the zero period work is uninterrupted by rest; in the first period ten seconds of work (i.e., ten lifts) alternate with one second of rest; in the second period ten seconds of work alternate with two seconds of rest and so on, as already indicated, until the seconds of work and rest are equal. Algebraically this may be represented by the formula $\frac{P}{W+R_0}$, $\frac{P}{W+R_1}$, $\frac{P}{W+R_2}$, - - - - - - - $\frac{P}{W+R_{10}}$, in which P = work period in in which each subdivision represents ten ergographic lifts; $W + R_0 =$ no rests between subdivisions; $W + R_1 = 1''$ rest between subdivisions and so on to $W + R_{10} = 10''$ rest between subdivisions. This procedure applies equally to the known and unknown series of experiments.

Excepting the third set of experiments, each set comprises four series (two known and two unknown) and each series comprises eleven subdivided work periods. The third set differs from the first and second in the sequence of the series, in the load, and in that the work periods number six. The first and second sets differ only in the sequence of the series.

---

[5] 'Exhaustion' as here used means nothing more than that the ergographic load could not be budged for two successive attempts; it certainly does not imply an anaesthetic condition of any part of the neuro-muscular chain. Moreover, it is doubtful whether anything like exhaustion is possible through voluntary stimulation.

First Set

I→
First Known, Ascending Series - - - - - - - - - - - Break
Number of the work periods 0 1 2 3 4 5 6 7 8 9 10
←II
First Unknown, Ascending Series - - - - - - - - - - -
no break
First Unknown, Descending Series - - - - - - - - - - -
←III
0 1 2 3 4 5 6 7 8 9 10 Break
First Known, Descending Series - - - - - - - - - - -
←IV

Second Set

V→
Second Unknown, Ascending Series - - - - - - - - - - - Break
0 1 2 3 4 5 6 7 8 9 10
VI→
Second Known, Ascending Series - - - - - - - - - - -
←VII no break
Second Known, Descending Series - - - - - - - - - - - Break
0 1 2 3 4 5 6 7 8 9 10
Second Unknown, Descending Series - - - - - - - - - - -
←VIII

Third Set (Isometric Contractions)

I→
First Known, Ascending Series - - - - - -
Number of the Work Periods 0 1 2 3 4 5
←II
First Unknown, Descending Series - - - - - -
III→
Second Known, Ascending Series - - - - - -
0 1 2 3 4 5
←IV
Second Known, Descending Series - - - - - -

Note: The broken lines represent the work periods of a series. The figures designate the duration in seconds of the rests in each work period and at the same time number the periods. The arrows indicate the order in which the work periods were performed.

It will be noticed that in the first set the two known occupy the least, and most, favorable positions with respect to practice effects. The two unknown series occupy a more favorable position than the first known series and a less favorable position than the second known series with respect to practice effects. This sequence permits a fairly equitable distribution of the effects of practice.

In the second set the two unknown and the two known series exchange positions with the two known and the two unknown series of the first set of experiments. The sequence of series in the first set and the reversal of the sequence (with respect to 'known' or 'unknown') in the second set distribute equally the advantages and disadvantages due to position.

In passing from the last work period of the *ascending* known of the first *set* to the zero work period of the *ascending* unknown of the same set a drop or 'break'; i.e. an abrupt transition from ten rests to no rests occurs. This is equally true of the first unknown and known of the descending series of the same set except that the break occurs in the reverse order. In this instance the transition is abrupt from a condition of no rest in the zero period to the maximum rest in the tenth period; in the former instance the transition or 'break' is from a condition of maximum rest in the tenth period to no rest in the zero period. We not only have to do here with the phenomenon of adaptation but adaptation under different conditions. This phenomenon of 'break' does not prevail between the two unknown ascending and descending (arrows, II & III, p. 6) series where the ascent of the one is followed by the gradual descent of the other. The same is true of the two known ascending and descending series (arrows VI & VII, p. 6) of the second set. Whatever advantages or disadvantages, if any, accrue to either the known or unknown series of the first set because of 'breaks' is fully covered by reversing the order of the series of the second set. We find, then, the first break of the first set between the first ascending known and unknown series paralleled by a break between the ascending unknown and known series of the second set. We find 'no break' between the two unknown series of the first set paralleled by 'no break' between the two known series of the second set. Further, the break between the descending unknown and known series of the first set is paralleled by a similar break in the descending known and unknown series of the second set.

In any ascending or descending series the subject observed a general adaptation or "attunement" of the system to rests as they increased or decreased. This is evident when it is observed (Table 5) that in the concluding work period of one of the ascending series there are 247 subdivisions (each subdivision comprises 10 lifts alternating with 10 seconds rest) with a total rest time of 41′ and a total work time of 41′, 1″. For this period the subject is in the machine 1 hr., 22 min., 1 sec. It should be noticed too that the immediately preceding work period covers a total time of 52 min. and 16 sec. From this condition of relative adaptation to long rest periods in an unknown series the subject begins the zero work period of a known series with no rest (Table 6). The brief duration, 2 min., 12 sec., of this work period stands in strong contrast to the immediately preceding work period mentioned above. The long interval of time separating work periods, obviously, obliterated much of the phenomenon of adaptation. That unmistakable traces of such adaptation remain, however, is evident from the subject's tendency to insert rests in the no-rest work period. In several instances such insertion actually occurs as the ergographic records show.

The intensity of the tendency to insert rests in the no-rest work period here under consideration (Table 6, zero period) is determined by the number and amount of rests which the subject made during the immediately preceding series of work periods (Table 5). Reference to this table shows that observer M rests 907 times in the eleven work days and that this amounts to 1 hour 58 min. and 53 seconds. As a consequence there is developed what may be called a *'periodic rest habit.'*

In commenting on the work of the zero period (Table 6) the observer says: "I can resist the tendency to rest here only with the greatest effort. In one or two instances I have actually rested as the records will no doubt show. I get an intense 'rest feel' at the conclusion of each tenth lift. This is interesting in that I seem driven by a sort of organic urgency amounting to inner necessity to insert the rests. How the rests are finally inhibited I am unable to explain."

What we have here is an advanced stage of habituation in which the tenth lift and a rest form fixed associates so that the appearance of the requisite number of lifts conditions the appearance of the rest. With a still more advanced stage of habituation the rests would be expected to follow the lifts more readily and persistently. The insertion of rests in a zero work period mentioned by the observer and indicated in the records clearly shows the presence of a conditioned reflex.

The mental complex takes on essentially new features when passing from the eleventh work period of the known series to the zero work period of the unknown series of the first set of experiments; similarly, when passing immediately from one extreme work period of any series, whether ascending or descending, to an opposite extreme of any other series, the conscious content accompanying such periods varies widely from that content common to consecutive work periods. Certain of these conscious features indicate the presence of unfavorable conditions for increased efficiency. Observer remarks: "I experience a peculiar depressing attitude, a distinct feeling of disinclination as I approach the concluding work periods of an ascending series. This feeling is quite the reverse of that characterizing a descending series. All extended work periods whether in ascending or descending series contain elements tending to decrease effort." The influence of such elements are reflected in the efficiencies of any two successive work periods which are separated by wide differences in the number of rest seconds. There is no way of definitely tracing the influence of such elements for the reason that we are dealing here with complex factors and it is undoubtedly true that the elements referred to are merged with such factors as motor attunement, practice effects and the like. It is probable, however, that practice effects exert a positive influence when a work period containing maximal rest seconds is followed by a work period of no rest, while the muscular "set" exerts a negative influence.

The sum for all observers of the absolute (W) values of the above compared work periods preceding the break in adaptation is 675.95 Kg M.; the sum for the periods immediately following the 'break' is 534.34 Kg M. The corresponding unit $\frac{W}{c}$ values are .8249 $\frac{\text{Kg M.}}{\text{Sec.}}$ and .8781 $\frac{\text{Kg M.}}{\text{Sec.}}$. From these data it follows that the periods immediately following the break in adaptation are less efficient by 141.61 Kg M. than the corresponding preceding periods. In terms of $\frac{\text{Kg M.}}{\text{Sec.}}$ the efficiency is reversed, that is, after the 'break' the periods immediately succeeding are more efficient by .0532 $\frac{\text{Kg M.}}{\text{Sec.}}$. The absolute values, therefore, stand in inverse relation to the unit values. What degree of differences here noted and to what extent the character of these differences can be ascribed to the phenomenon of adaptation would be difficult to say because of the multiplicity of factors involved. Practice effects and awareness and lack of awareness of results without doubt condition the values here.

The following data taken from observer M are illustrative of the compared periods:

First Break of First Set of Experiments:

Zero period, 1st Asc. Kn. Series —	6.66	Kg M. —	.0569	$\frac{\text{Kg M.}}{\text{Sec.}}$
" " 1st " Unkn. " —	8.65	" —	.1008	"
	1.99	"	.0439	"

Second Break of First Set of Experiments:

10th per., 1st Desc. Unkn. Series —	111.37	Kg M. —	.0918	$\frac{\text{Kg M.}}{\text{Sec.}}$
" " " " Kn. " —	97.36	" —	.0764	"
	14.01	"	.0154	"

First Break of Second Set of Experiments:

Zero per., 2nd. Asc. Unkn. Series —	12.00	Kg M. —	.0857	$\frac{\text{Kg M.}}{\text{Sec.}}$
" " " " Kn. " —	11.64	" —	.0883	"
	.36	"	.0026	"

Second Break of Second Set of Experiments:

10th per., 2nd. Desc. Kn. Series —	233.23	Kg M. —	.0923	$\frac{\text{Kg M.}}{\text{Sec.}}$
" " " " Unkn. " —	132.06	" —	.0999	"
	101.17	" —	.0076	"

It has already been noted that the arrangement of the various series in the first and second sets of experiments is intended to distribute equally the advantages and disadvantages to the known and unknown series.

The third set of experiments effectually eliminates all breaks between series whether known or unknown. The muscular adaptation to gradually increasing rest in an ascending series is followed by an adaptation to gradually decreasing rest in a descending series. In this set of experiments as in the first set the unknown series occupy the least, and most, favorable positions. The maximum rest in any subdivision of a series is five seconds while the minimum is zero as in the preceding sets. A fourth set, bearing the same relation to the third set as the second set bears to the first, was made impossible by the withdrawal of the observers. The ergographic load for this third set was six kilograms while for the first and second sets it was four kilograms.

The isometric method (spring) was employed throughout for both loads. The Bergström ergograph is adapted to this method although it could be made to serve the isotanic method.[6]

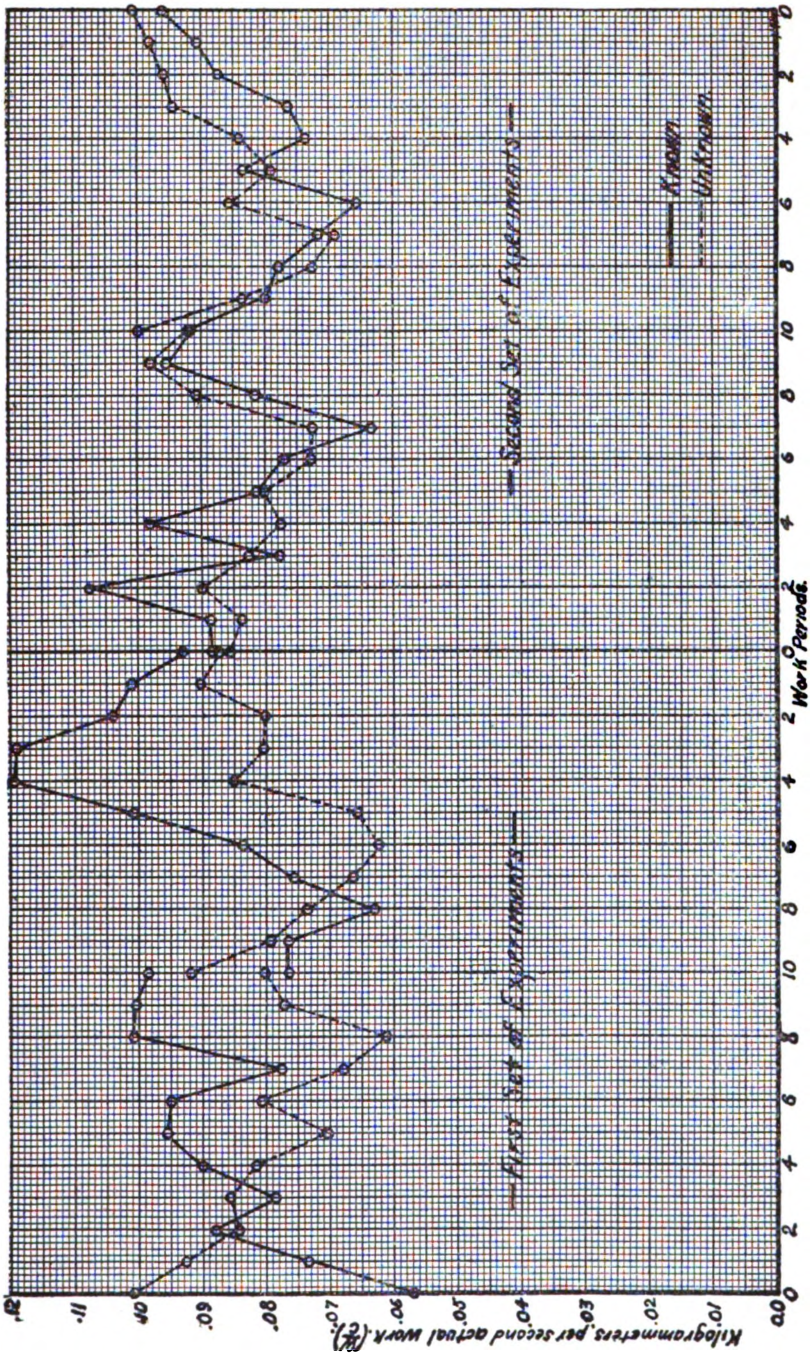
RESULTS

In the three sets of experiments there are six unknown and six known series which together constitute one hundred twelve work periods. Each known work period has a corresponding unknown period, and each period, whether known or unknown, has an *absolute* and a *unit* efficiency value. For example, in Table I the absolute and unit values of the fifth work period of the ascending known series are 22.84 kilogrammeters and .0954 kilogrammeter seconds (Kg M/Sec.) respectively. The corresponding values in the ascending unknown series are, 22.94 Kg M. and .0703 Kg M/sec. (Table II) In this particular case the absolute values are equivocal while the unit value of the known clearly exceeds that of the unknown. If now the remaining periods are considered in the same manner we get the following variety of relations:

1. In forty-six cases the absolute and unit values of the known periods stand in direct relation to the corresponding values of the unknown periods. In these cases the two kinds of values of the known periods exceed that of the unknown.

---

[6] The literature setting forth the merits of the two methods is cited by T. A. Storey in the Amer. Journ. of Physiol. Vol. 8, p. 358.



Curve I. (Table I to VIII inclusive).

TABLES I AND II

First Ascending Known. Load 4 Kilograms (Observer M).

B	C		I	III		C+III		D	W	$\frac{W}{C}$	$\frac{W}{C+III}$	MWJ	WP
	min.	sec.		min.	sec.	min.	sec.						
1	1	57	0	0	0	1	57	1.665	6.66	.0569	.0569	.0595	.0595
12	1	53	1	0	11	2	4	2.073	8.29	.0733	.0669	.0805	.0737
11	1	46	2	0	20	2	6	2.330	9.32	.0879	.0740	.0878	.0735
12	1	56	3	0	33	2	29	2.276	9.10	.0785	.0610	.0854	.0338
21	3	37	4	1	24	5	1	4.877	19.51	.0899	.0649	.0828	.0605
24	3	59	5	1	55	5	54	5.710	22.84	.0954	.0646	.0883	.0614
23	3	49	6	2	12	6	1	5.447	21.79	.0949	.0603	.0859	.0549
32	5	16	7	3	37	8	53	6.144	24.57	.0776	.0461	.0789	.0481
32	5	27	8	4	8	9	35	8.482	33.93	.1008	.0589	.0908	.0627
54	8	59	9	7	57	16	56	13.921	55.68	.1003	.0548	.0917	.0497
55	9	17	10	9	0	18	17	13.693	54.77	.0985	.0500	.0917	.0465
First Ascending Unknown. Load 4 Kilograms.													
1	1	20	0	0	0	1	20	2.162	8.65	.1008	.1008	.0705	.0725
11	1	54	1	0	10	2	4	2.642	10.57	.0926	.0851	.0893	.0788
13	2	18	2	0	24	2	42	2.905	11.62	.0843	.0717	.0876	.0739
15	2	25	3	0	42	3	7	3.101	12.40	.0856	.0664	.0747	.0595
21	3	35	4	1	20	4	55	4.382	17.53	.0816	.0593	.0707	.0579
33	5	26	5	2	40	8	6	5.734	22.94	.0703	.0471	.0714	.0479
21	3	35	6	2	0	5	35	4.339	17.36	.0806	.0518	.0708	.0454
40	6	37	7	4	33	11	10	6.746	26.98	.0679	.0402	.0677	.0407
64	10	40	8	8	24	19	4	9.781	39.12	.0612	.0342	.0624	.0350
101	16	52	9	15	0	31	52	19.528	78.11	.0771	.0408	.0658	.0350
90	15	0	10	15	0	30	0	17.584	70.34	.0781	.0407	.0692	.0354

B = Number of subdivisions of work periods.
C = Total time at work.
I = Length of rest period in seconds and designation of the period.[1]
III = Total rest time.
C+III = Total time of work and rest.
D = Distance load travels in meters.
W = Work done in kilogrammeters.
$\frac{W}{C}$ = Unit value per second of work done each period.
$\frac{W}{C+III}$ = Rate of work including the rest time.
MWJ = Average unit value ($\frac{W}{c}$) of all observers.
WP = Average rate of work per second for each period including the rest time.

---

[1] Attention is called to the fact that the periods, 0, 1, 2, 3, 4 - - - - 10 are the 1st, 2nd, 3rd, 4th, 5th - - - - 11th in the order of performance respectively.

TABLES III AND IV

B	C		I	III		C+III		D	W	$\frac{W}{C}$	$\frac{W}{C+III}$	MWJ	WP
	min.	sec.		min.	sec.	min.	sec.						
	First Descending Unknown.							Load 4 Kilograms. (Observer M)					
122	20	13	10	20	10	40	23	27.843	111.37	.0918	.0458	.0814	.0383
81	13	30	9	12	0	25	30	16.072	64.29	.0794	.0419	.0803	.0409
89	14	46	8	11	44	26	30	16.297	65.19	.0736	.0410	.0669	.0373
121	20	11	7	14	0	34	11	16.560	66.24	.0663	.0323	.0627	.0351
51	9	36	6	5	36	15	12	8.989	35.96	.0624	.0394	.0608	.0383
55	9	16	5	4	30	13	46	9.160	36.64	.0659	.0445	.0612	.0414
29	4	55	4	1	52	6	47	6.262	25.05	*.0850*	.0616	.0662	.0382
24	4	6	3	1	9	5	15	4.951	19.80	.0804	.0629	.0723	.0564
19	3	16	2	0	36	3	52	3.925	15.70	.0801	.0681	.0759	.0648
19	3	12	1	0	19	3	30	4.341	17.36	.0904	.0826	.0768	.0704
1	2	2	0	0	0	2	2	2.660	10.64	.0873	.0873	.0713	.0700
	First Descending Known.							Load 4 Kilograms.					
128	21	14	10	21	10	42	24	24.340	97.36	.0764	.0382	.0804	.0404
88	14	34	9	13	3	27	37	16.717	66.87	.0764	.0404	.0793	.0418
126	20	55	8	16	40	37	35	10.781	79.12	.0630	.0351	.0675	.0377
98	16	15	7	11	19	27	34	18.389	73.56	.0755	.0445	.0761	.0453
59	9	43	6	5	48	15	31	12.200	48.80	.0837	.0524	.0824	.0518
93	15	22	5	7	40	23	2	24.500	98.00	.1006	.0709	.0735	.0500
67	11	6	4	4	24	15	30	14.964	79.86	*.1195*	.0859	.0793	.0572
24	4	0	3	1	9	5	9	7.108	28.43	.1190	.0923	.0963	.0748
20	3	13	2	0	38	3	51	5.017	20.07	.1040	.0872	.0951	.0803
16	2	33	1	0	15	2	48	3.863	15.45	.1010	.0920	.0832	.0763
1	2	0	0	0	0	2	0	2.795	11.18	.0930	.0930	.0783	.0782

2. In twenty-three cases the absolute and unit values of the unknown periods stand in direct relation to the corresponding values of the known periods. In these cases the two kinds of values of the unknown periods exceed that of the known.

3. In thirty-one cases the unit values of the known work periods stand in inverse relation to the corresponding absolute values of the unknown periods. In these cases the unit values of the known work periods and the absolute values of the unknown work periods are more efficient than their corresponding values. For example, consider the third work period (Table 1). Here the absolute (W) value is 9.32 Kg M. and the unit $(\frac{W}{c})$ value is $.0879 \frac{\text{Kg M.}}{\text{Sec.}}$ for the known series of work periods. The corresponding values (Table 11) for the unknown series are 11.62 Kg M. and $.0843 \frac{\text{Kg M.}}{\text{Sec.}}$. The unit value $(.0879 \frac{\text{Kg M.}}{\text{Sec.}})$

of the known and the absolute value (11.62 Kg M.) of the unknown series are more efficient than their respective corresponding values, (.0843 $\frac{\text{Kg M.}}{\text{Sec.}}$ and 9.32 Kg M.)

4. In twenty-one cases the unit values of the known work periods stand in inverse relation to the corresponding absolute values of the unknown periods. In these cases, however, the unit values of the known work periods and the absolute values of the unknown work periods are less efficient than their corresponding values.

The following summary of Tables III and IV is illustrative of the treatment of all the tables as summarized above:

First Descending Known and First Descending Unknown Series.

	Absolute values of known work periods indicated with symbols, greater (>) or less (<) than the corresponding unknown values.	*Unit* values of known work periods indicated with symbols, greater (>) or less (<) than the corresponding unknown values.
0 work period	<	>
1 " "	<	>
2 " "	>	>
3 " "	>	>
4 " "	>	>
5 " "	>	>
6 " "	>	>
7 " "	>	>
8 " "	>	<
9 " "	>	<
10 " "	<	<

Periods 0 and 1 are illustrative of the thirty-one cases above. Periods 2, 3, 4, 5, 6, and 7 are illustrative of the forty-six cases above. Periods 8 and 9 are illustrative of the twenty-one cases above. Period 10 is illustrative of the twenty-three cases above.

Of the total number of cases (121) 38 per cent show a higher efficiency value, absolute and unit, for the known as compared to 19 per cent of the unknown work periods. In 26 per cent of the cases the unit values of the known and the absolute values of the unknown show a higher efficiency, while the unit values of the unknown and the absolute values of the known show a high efficiency in 17 per cent of the cases. Combining the above percentages we find 64 per cent of the unit values and 55 per cent of the absolute values of the known work periods show a higher efficiency than the corresponding values for the unknown periods.

If the conditions of the experiment secure an equitable distribution of the complex factors involved in each work period such as adaptation, fatigue, ergographic defects and the like, then we may conclude that the increased efficiency of the known periods over that of the unknown is conditioned by the conscious factor of awareness of results. This means that the organism functions more effectively when the ergographic tracings are immediately present stimuli.

The influence of this factor may be found by comparing the absolute and unit efficiency values for the known and unknown periods as given in Table I to VIII and XIII to XIX inclusive. A summary is given in Table XXII.

In three work periods the average unit values (WP) of the unknown periods exceed that of the known. These are the 0, 2, and 3 periods and are indicated by the less sign (<) in Table XX. In one work period only do the average unit values ($\frac{W}{c}$ for M W J) of the unknown exceed in efficiency those of the known periods. This is the zero period and is indicated by the less sign (<) in Table XX.

The relation of the grand averages of the various efficiency values for all the work periods of the known and unknown series is found in Table XXII.

As Table XX shows the average unit values of the various series so Table XXI shows the average absolute values for the same series. Reference to Table XXI shows that in two work periods only (0 and 1) do the average absolute values of the unknown exceed that of the known periods. An average of all the work periods of all the observers shows the known series to excel in absolute efficiency that of the unknown by .84 Kg M. (Table XXII). A similar average of the unit values shows the known series to excel that of the unknown by .0053 $\frac{\text{Kg M.}}{\text{Sec.}}$ (Table XXII).

TABLES V AND VI.

Second Ascending Unknown. Load 4 Kilograms. (Observer M)

B	C		I	III		C+III		D	W	W/C	W/C+III	MWJ	WP
	min.	sec.		min.	sec.	min.	sec.						
1	2	20	0	0	0	2	20	3.000	12.00	.0857	.0857	.0814	.0817
20	3	15	1	0	18	3	33	4.090	16.36	.0838	.0767	.0769	.0704
16	2	44	2	0	30	3	14	3.468	13.87	.0900	.0714	.0873	.0731
32	5	16	3	1	33	6	49	6.509	26.04	.0825	.0637	.0847	.0641
31	5	6	4	2	0	7	6	5.925	23.70	.0774	.0556	.0657	.0473
70	11	32	5	9	55	21	27	13.834	55.34	.0800	.0430	.0663	.0414
82	13	33	6	8	6	21	39	14.812	59.24	.0729	.0456	.0710	.0446
83	13	42	7	9	34	23	16	14.880	59.52	.0726	.0426	.0696	.0410
160	26	30	8	21	12	47	42	35.097	140.39	.0907	.0496	.0726	.0404
166	27	31	9	24	45	52	16	40.385	161.54	.0979	.0515	.0864	.0459
247	41	1	10	41	0	82	1	56.586	226.34	.0920	.0461	.0761	.0448

Second Ascending Known. Load 4 Kilograms.

B	C		I	III		C+III		D	W	W/C	W/C+III	MWJ	WP
1	2	12	0	0	0	2	12	2.910	11.64	.0883	.0883	.0820	.0816
17	2	48	1	0	16	3	4	3.721	14.88	.0886	.0809	.0875	.0800
19	3	10	2	0	36	3	46	5.108	20.43	.1075	.0906	.0883	.0747
28	4	36	3	0	21	5	57	5.381	21.52	.0778	.0606	.0750	.0586
31	5	4	4	2	0	7	4	7.446	29.78	.0981	.0701	.0726	.0532
42	6	55	5	3	25	10	20	8.462	33.85	.0814	.0546	.0663	.0446
85	14	3	6	8	24	22	27	16.235	64.94	.0770	.0482	.0625	.0404
147	24	21	7	17	2	41	23	23.173	92.69	.0634	.0373	.0679	.0383
189	31	5	8	25	4	56	9	38.363	153.45	.0815	.0455	.0844	.0474
209	34	50	9	31	12	66	2	49.757	199.03	.0953	.0504	.0864	.0409
262	43	33	10	43	30	87	3	61.160	244.64	.0917	.0470	.0785	.0397

TABLES VII AND VIII.

Second Descending Known. Load 4 Kilograms. (Observer M)

B	C		I	III		C+III		D	W	W/C	W/C+III	MWJ	WP[1]
	min.	sec.		min.	sec.	min.	sec.						
263	43	50	10	43	40	*87*	*30*	58.308	233.23	*.0923*	.0443	.0788	.0585
202	33	40	9	30	9	*63*	*49*	40.389	161.56	*.0800*	.0422	.0745	.0393
142	23	31	8	18	48	42	19	27.474	109.90	.0778	.0399	.0688	.0355
94	15	42	7	11	21	27	3	16.887	67.55	.0718	.0417	.0785	.0460
106	18	37	6	10	30	28	7	17.365	69.46	.0656	.0411	.0687	.0431
64	10	31	5	5	15	15	46	13.147	52.59	.0833	.0556	.0783	.0524
48	7	54	4	8	3	15	57	8.730	34.92	.0737	.0364	.0664	.0392
39	6	13	3	1	51	8	4	6.861	27.44	.0763	.0569	.0648	.0493
17	2	42	2	0	32	3	14	3.537	14.15	.0872	.0729	.0786	.0443
15	2	22	1	0	14	2	36	3.220	12.88	.0907	.0827	.0886	.0809
1	1	55	0	0	0	0	55	2.765	11.00	.0960	.0960	.0898	.0898

Second Descending Unknown. Load 4 Kilograms.

B	C		I	III		C+III		D	W	W/C	W/C+III	MWJ	WP[1]
133	22	1	10	22	0	44	1	33.015	132.06	.0999	.0498	.0896	.0449
160	26	31	9	23	51	50	22	33.347	133.39	.0835	.0322	.0768	.0305
134	22	12	8	17	44	39	56	24.145	96.58	.0725	.0404	.0648	.0361
156	25	52	7	18	5	43	57	26.778	107.11	.0689	.0407	.0656	.0387
118	19	40	6	11	42	31	22	25.204	100.81	.0854	.0535	.0799	.0502
73	12	3	5	6	0	18	3	14.437	57.75	.0789	.0533	.0672	.0456
63	10	27	4	4	8	14	35	13.157	52.63	.0840	.0602	.0710	.0493
30	4	51	3	1	27	6	18	6.855	21.42	.0943	.0726	.0730	.0564
22	3	34	2	0	42	4	16	5.134	20.54	.0958	.0802	.0879	.0738
16	2	38	1	0	15	2	53	3.870	15.48	.0980	.0895	.0842	.0767
1	1	50	0	0	0	1	50	2.766	11.06	.1005	.1005	.0957	.0957

[1] WP does not include the data of Observer J for the second descending 'known' and 'unknown' series of work periods.

Table IX.

First Ascending Known Series. Load 6 Kilograms. (Observer M.)

B	C		I	III		C+III		C	W	$\frac{W}{C}$
	Min.	Sec.		Min.	Sec.	Min.	Sec.			
1	1	5	0	0	0	1	5	1.513	7.88	.1029
9	1	33	1	0	8	1	41	1.700	10.20	.1098
10	1	35	2	0	18	1	53	2.193	13.16	.1382
10	1	40	3	0	27	2	7	2.380	14.28	.1428
12	1	53	4	0	44	2	37	2.488	14.93	.1320
11	1	54	5	0	50	2	44	2.407	14.44	.1268

Table X.

First Descending Unknown Series. Load 6 Kilograms.

13	2	2	5	1	0	3	2	2.760	16.56	.1360
10	1	40	4	0	36	2	16	2.372	14.23	.1423
10	1	34	3	0	27	2	1	1.880	11.28	.1198
9	1	25	2	0	16	1	41	2.003	12.03	.1420
7	1	4	1	0	6	1	10	1.517	8.10	.1420
1	1	1	0	0	0	1	1	1.410	8.46	.1386

Table XI.

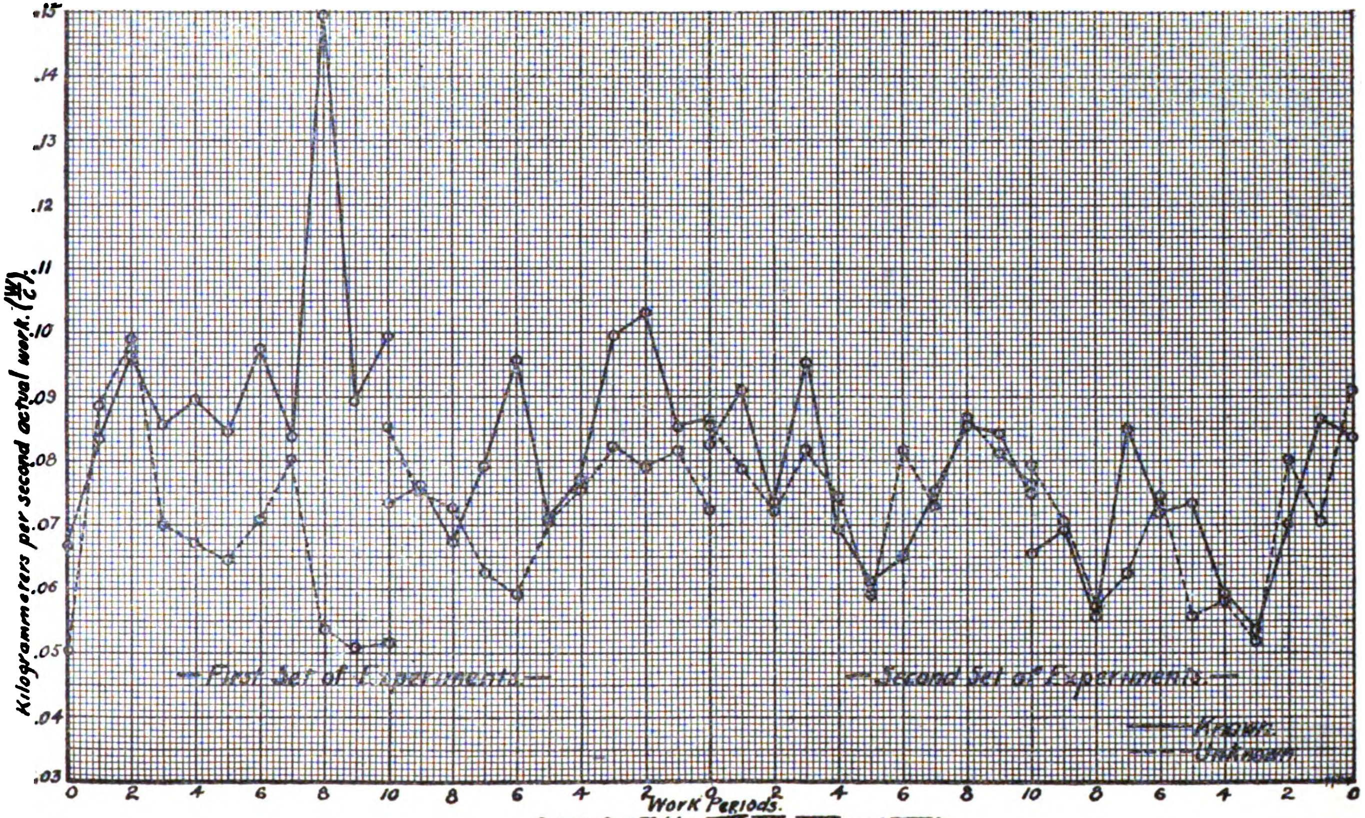
Second Ascending Known Series. Load 6 Kilograms.

1	0	49	0	0	0	0	49	1.474	8.84	.1822
9	1	24	1	0	8	1	32	2.026	12.16	.1447
9	1	23	2	0	16	1	39	1.955	11.73	.1414
13	2	2	3	0	36	2	38	1.765	10.59	.0868
15	2	22	4	0	56	3	18	3.325	19.95	.1404
13	2	3	5	1	0	3	3	2.116	12.70	.1003

Table XII.

Second Descending Known Series. Load 6 Kilograms.

14	2	14	5	1	5	3	19	3.200	19.20	.1433
14	2	11	4	0	52	3	3	3.025	18.15	.1384
12	1	53	3	0	33	2	26	2.712	16.27	.1442
8	1	20	2	0	14	1	34	2.136	12.82	.1602
9	1	22	1	0	8	1	30	2.127	12.76	.1556
1	1	6	0	0	0	1	0	1.735	10.41	.1578



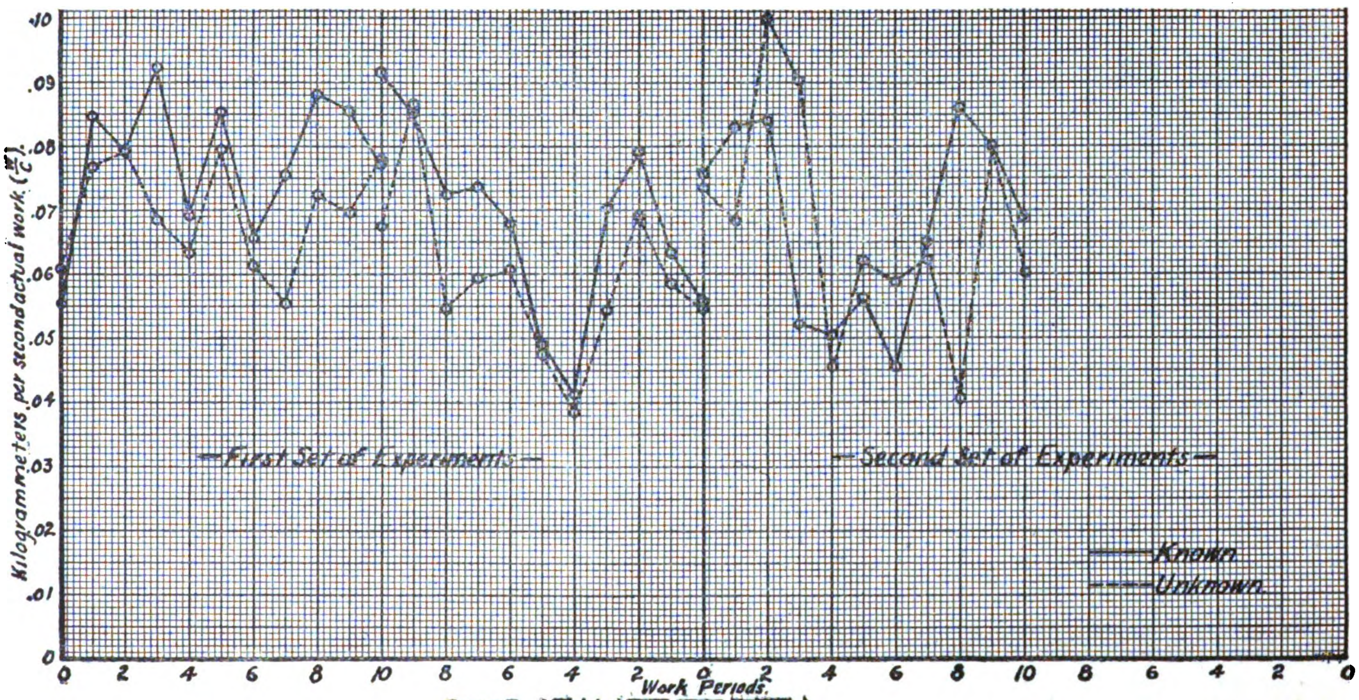
Curve 2. (Tables XIII, XV, XVII, and XIX).

TABLE XIII.

B	C		I	III		C+III		D	W	W / C	W / C+III
	Min.	Sec.		Min.	Sec.	Min.	Sec.				
First Ascending Known. Load 4 Kilograms. (Observer W.)											
1	1	32	0	0	0	1	32	1.53	6.12	.0665	.0665
9	1	35	1	0	8	1	43	1.98	7.92	.0833	.0768
11	1	40	2	0	20	2	0	2.41	9.64	.0964	.0800
10	1	35	3	0	27	2	2	2.03	8.12	.0854	.0665
15	2	34	4	0	56	3	30	3.45	13.80	.0896	.0657
14	2	28	5	1	5	3	33	3.12	12.48	.0843	.0585
21	3	37	6	2	–	5	37	5.29	21.16	.0975	.0627
29	4	52	7	3	16	7	68	6.21	24.84	.0838	.0501
36	4	1	8	4	40	8	41	9.01	36.04	*.1495*	.0691
30	5	9	9	4	21	9	30	6.90	27.60	.0893	.0484
43	7	15	10	7	0	14	15	10.82	43.28	.0994	.0505
First Ascending Unknown. Load 4 Kilograms.											
1	1	53	0	–	–	1	53	1.42	5.68	.0502	.0502
10	1	40	1	–	9	1	49	2.21	8.84	.0884	.0811–
8	1	22	2	–	14	1	36	2.03	8.12	.0990	.0845–
15	2	38	3	–	42	3	20	2.76	11.06	.0700	.0553–
12	2	8	4	–	44	2	52	2.15	8.60	.0671	.0500–
18	3	4	5	1	35	4	39	2.97	11.88	.0645	.0425–
23	3	58	6	2	12	6	10	4.21	16.84	.0707	.0455–
39	6	32	7	4	26	10	58	2.83	31.36	.0800	.0476
40	6	45	8	5	12	11	57	5.42	21.68	*.0535*	.0302
54	9	3	9	7	51	17	–	6.92	27.68	.0509	.0272
63	10	39	10	10	20	20	50	8.24	32.96	.0515	.0263

TABLE XIV.

B	C		I	III		C+III		D	W	W / C	W / C+III
	Min.	Sec.		Min.	Sec.	Min.	Sec.				
First Ascending Known. Load 4 Kilograms. (Observer J.)											
1	1	48	0	0	0	1	48	1.49	5.96	.0552	.0551
12	1	54	1	0	11	2	5	2.42	9.68	.0848	.0774
10	1	41	2	0	18	1	59	1.98	7.92	.0792	.0665
13	2	18	3	0	36	2	54	3.18	12.72	.0922	.0731
18	3	9	4	1	8	4	17	3.26	13.04	.0691	.0509
22	3	47	5	1	45	5	32	5.08	20.32	.0851	.0612
20	3	25	6	1	55	5	20	3.35	13.40	.0654	.0418
29	4	58	7	3	16	8	6	5.61	22.44	.0753	.0461
28	4	43	8	3	36	8	16	6.24	24.95	.0882	.0503
44	7	27	9	6	27	13	47	9.54	38.17	.0854	.0461
54	9	9	10	8	50	17	59	10.58	42.32	.0771	.0390
First Ascending Unknown. Load 4 Kilograms.											
1	1	30	0	0	0	1	30	1.50	6.00	.0606	.0666
11	1	52	1	0	10	2	2	2.15	8.60	.0769	.0704
15	2	36	2	0	28	3	4	3.02	12.09	.0795	.0657
21	3	31	3	1	0	4	31	3.61	14.45	.0685	.0570
15	2	32	4		56	3	28	3.36	13.44	.0634	.0646
28	4	49	5	2	15	7	4	5.73	22.94	.0794	.0541
47	7	56	6	4	36	12	26	7.28	29.13	.0612	.0390
42	7	4	7	4	47	11	51	6.13	24.54	.0553	.0345
54	9	7	8	7	4	16	11	9.90	39.60	.0724	.0407
68	11	28	9	10	3	21	31	11.97	47.88	.0696	.0370
70	11	42	10	11	30	23	12	13.68	54.75	.0780	.0393



Curve 3. (Tables XIV, XVI, XVIII).

TABLE XV.

B	C		I	III		C+III		D	W	W	W
	Min.	Sec.		Min.	Sec.	Min.	Sec.			C	C+III
First Descending Known. Load 4 Kilograms (Observer W).											
93	15	36	10	15	20	30	56	17.17	68.70	.0734	.0370
79	13	15	9	11	42	24	57	15.14	60.58	.0762	.0405
90	15	9	8	11	52	27	1	15.29	61.17	.0673	.0377
65	10	56	7	7	28	18	24	13.20	52.78	.0791	.0478
60	10	8	6	5	54	16	2	14.53	58.12	.0956	.0605
75	12	33	5	6	10	18	43	13.40	53.60	.0710	.0477
50	8	28	4	3	16	11	44	9.80	39.22	.0772	.0557
38	6	25	3	1	51	8	16	9.58	38.31	.0995	.0772
15	2	37	2	0	28	3	5	4.00	16.01	.1030	.0865
8	1	23	1	0	7	1	30	1.77	7.06	.0851	.0784
1	1	20	0	0	0	1	20	1.73	6.90	.0863	.0862
First Descending Unknown. Load 4 Kilograms.											
85	14	10	10	14	0	28	10	18.08	72.34	.0851	.0360
50	8	27	9	7	21	15	48	9.51	38.03	.0750	.0401
53	8	56	8	6	56	15	52	9.70	38.81	.0724	.0407
86	14	29	7	9	55	24	24	13.58	54.31	.0625	.0371
70	11	42	6	6	54	18	36	10.39	41.56	.0592	.0372
45	7	35	5	3	40	11	15	7.99	31.94	.0702	.0473
15	2	36	4	0	56	3	32	2.94	11.75	.0753	.0554
22	3	41	3	1	3	4	44	4.54	18.14	.0821	.0639
10	1	46	2	0	18	2	4	2.09	8.35	.0788	.0673
12	2	4	1	0	11	2	15	2.53	10.11	.0815	.0748
1		58	0	0	0	0	58	1.05	4.18	.0721	.0720

TABLE XVI.

B	C		I	III		C+III		D	W	W	W
	Min.	Sec.		Min.	Sec.	Min.	Sec.			C	C+III
First Descending Known. Load 4 Kilograms (Observer J).											
110	18	29	10	18	10	36	39	25.37	101.47	.0915	.0461
83	13	55	9	12	18	26	13	18.29	73.14	.0852	.0465
95	15	56	8	12	32	28	28	17.23	68.93	.0721	.0403
62	10	23	7	7	7	17	30	11.46	45.85	.0736	.0438
70	11	48	6	6	54	18	42	12.00	48.00	.0678	.0427
32	5	22	5	2	35	7	57	3.94	15.75	.0489	.0314
25	4	17	4	1	36	5	53	2.65	10.61	.0413	.0300
24	4	9	3	1	9	5	18	4.37	17.50	.0703	.0550
10	1	42	2		18	2	00	2.02	8.08	.0792	.0673
6	1	3	1		5	1	8	1.00	3.98	.0632	.0585
1	1	20	0			1	20	1.11	4.44	.0555	.0555
First Descending Unknown. Load 4 Kilograms.											
98	16	28	10	16	20	32	48	16.60	66.40	.0674	.0332
115	19	12	9	17	6	36	18	24.91	99.65	.0865	.0411
100	16	41	8	13	20	30	1	13.66	54.65	.0546	.0303
43	7	17	7	4	44	10	1	6.48	25.91	.0593	.0359
55	9	16	6	5	24	14	40	8.44	33.75	.0607	.0383
22	3	43	5	1	45	5	28	2.66	10.66	.0476	.0324
18	3	4	4	1	8	4	12	1.74	6.97	.0384	.0276
21	3	37	3	1	00	4	37	2.95	11.78	.0543	.0425
8	1	25	2		14	1	39	1.47	5.87	.0690	.0592
12	2	8	1		11	2	19	1.87	7.48	.0584	.0538
1	1	52	0			1	52	1.53	6.10	.0545	0.508

TABLE XVII

Second Ascending Known. Load 4 Kilograms. (Observer W).

B	C		I	III		C+III		D	W	$\frac{W}{C}$	$\frac{W}{C+III}$
	Min.	Sec.		Min.	Sec.	Min.	Sec.				
1	1	58	0	0	0	1	58	2.42	9.71	.0823	.0822
18	3	3	1	0	17	3	20	4.16	16.65	.0910	.0832
12	2	7	2	0	22	2	29	2.33	9.33	.0735	.0632
25	4	15	3	1	12	5	27	6.07	24.28	.0952	.0742
24	4	4	4	1	32	5	36	4.23	16.93	.0694	.0509
39	6	38	5	3	10	9	48	6.99	24.40	.0613	.0415
75	12	31	6	7	24	19	55	12.22	48.89	*.0651*	.0409
54	9	5	7	6	11	15	16	10.20	40.78	.0752	.0445
90	15	4	8	11	52	26	56	19.60	78.38	.0856	.0485
120	20	7	9	17	51	37	58	25.42	101.67	.0841	.0446
193	32	13	10	32	00	64	13	36.19	144.78	.0749	.0375

Second Ascending Unknown. Load 4 Kilograms.

B	C		I	III		C+III		D	W	$\frac{W}{C}$	$\frac{W}{C+III}$
1	2	5	0	0	0	2	5	2.64	10.65	.0852	.0852
20	3	23	1	0	19	3	42	3.98	15.94	.0785	.0718
10	1	48	2	0	18	2	6	1.94	7.78	.0720	.0633
18	3	20	3	0	51	4	11	3.71	14.83	.0815	.0581
38	6	25	4	2	28	8	53	7.13	28.53	.0741	.0535
63	10	37	5	5	10	15	47	9.06	36.25	.0569	.0380
59	9	51	6	5	48	15	39	12.06	48.23	*.0816*	.0513
85	14	17	7	9	48	24	5	15.83	63.33	.0739	.0437
81	13	31	8	10	40	24	11	17.60	70.39	.0868	.0492
160	26	49	9	23	50	50	39	32.66	130.65	.0812	.0429
174	29	4	10	28	50	57	54	33.31	133.24	.0764	.0383

TABLE XVIII

Second Ascending Known. Load 4 Kilograms (Observer J).

B	C		I	III		C+III		D	W	$\frac{W}{C}$	$\frac{W}{C+III}$
	Min.	Sec.		Min.	Sec.	Min.	Sec.				
1	1	42	0			1	42	1.92	7.69	.0754	.0753
15	2	35	1		14	2	49	3.22	12.86	.0830	.0760
25	4	16	2		48	5	4	5.38	21.50	.0840	.0703
18	3	7	3		51	3	58	2.44	9.74	*.0521*	.0409
65	10	59	4	3	16	14	15	8.27	33.08	.0502	.0385
30	5	1	5	2	25	7	26	4.22	16.89	.0563	.0378
69	11	37	6	4	48	16	25	7.89	31.57	.0453	.0320
110	18	23	7	12	33	30	56	17.92	71.69	.0650	.0332
85	14	18	8	11	12	25	30	18.47	73.87	*.0861*	.0482
160	26	41	9	23	51	50	32	21.17	84.67	.0798	.0278
132	22	3	10	21	50	43	53	22.76	91.02	.0688	.0345

Second Ascending Unknown. Load 4 Kilograms (Observer W).

B	C		I	III		C+III		D	W	$\frac{W}{C}$	$\frac{W}{C+III}$
1	1	39	0			1	39	1.81	7.27	.0734	.0734
10	1	46	1		9	1	55	1.81	7.24	.0683	.0629
11	1	52	2		20	2	12	2.80	11.19	.0999	.0847
24	4	9	3	1	9	5	18	5.61	22.46	*.0902*	.0706
32	5	28	4	2	4	7	32	3.74	74.96	.0456	.0330
88	14	42	5	6	25	21	7	13.69	54.77	.0621	.0432
52	8	44	6	5	6	13	50	7.67	30.71	.0586	.0369
90	15	6	7	10	32	25	38	14.13	56.53	.0624	.0367
123	20	33	8	16	16	36	49	12.39	49.57	*.0402*	.0224
108	17	29	9	16	3	33	32	21.81	87.23	.0801	.0433
144	24	.5	10	23	50	47	55	21.67	86.70	.0600	.0302

TABLE XIX

B	C		1	III		C+III		D	W	$\frac{W}{C}$	$\frac{W}{C+III}$
	Min.	Sec.		Min.	Sec.	Min.	Sec.				
Second Descending Known. Load 4 Kilograms (Observer W).											
220	36	41	10	36	30	73	11	35.99	143.95	.0654	.0327
175	29	15	9	26	6	55	21	30.32	121.27	.0691	.0365
176	29	26	8	23	20	52	46	24.64	98.54	.0558	.0311
82	13	42	7	9	27	23	9	17.51	70.03	.0852	.0504
91	15	15	6	9	00	24	15	16.45	65.79	.0719	.0452
55	9	13	5	4	30	13	43	10.13	40.53	.0733	.0492
31	5	19	4	2	00	7	19	4.72	18.88	.0592	.0430
23	3	52	3	1	6	4	58	3.10	12.39	.0534	.0418
8	1	24	2	0	14	1	38	1.47	5.89	.0701	.0601
15	2	31	1	0	14	2	45	3.27	13.08	.0866	.0792
1	1	51	0	0	0	1	51	2.32	9.29	.0837	.0836
Second Descending Unknown. Load 4 Kilograms.											
108	18	8	10	17	50	35	58	21.59	86.38	.0793	.0400
109	18	13	9	16	12	34	25	19.18	76.73	.0702	.0289
93	15	37	8	12	16	27	53	13.35	53.40	.0571	.0319
101	16	56	7	11	40	28	36	15.82	63.30	.0623	.0368
89	14	59	6	8	48	23	47	16.74	66.98	.0745	.0469
62	10	21	5	5	5	15	26	8.61	34.47	.0555	.0379
48	8	9	4	3	8	12	17	7.10	28.41	.0581	.0385
21	3	32	3	1	00	4	32	2.74	10.96	.0517	.0402
18	3	5	2	0	34	3	39	3.70	14.80	.0800	.0675
9	1	34	1	0	8	1	42	1.65	6.62	.0704	.0640
1	1	42	0	0	0	1	42	2.32	9.28	.0910	.0909

Of the two values the absolute is less significant, for it is highly improbable that the time rate for the work periods of the known corresponds to that of the unknown. There is no technique, controlling the various incentives to effort, operative for any series. Moreover, there is no absolute assurance that the instruction to make each ergographic lift represent maximal effort actually prevailed. A slower time rate of work may offer opportunity for partial recovery from fatigue and this would be equivalent to an increased period of rest. The absolute values are important here, therefore, in so far as they support the unit values.

A further comparison of the values of the known with the unknown series yields the following results:

1. The final average of the absolute efficiency values of all the known series of the first and second sets of experiments exceeds that of the unknown by .2 percent.

2. The final average of the unit efficiency values of all the known series, excepting the third set of experiments, excels that

of the unknown by 7 percent. In the known work periods the observer worked at a higher rate of speed.

In comparing the data of the third set of experiments of observer M (Table XXII) with the first and second sets we find the following:

(a) The average absolute efficiency value of all the known series exceeds that of the unknown by 10 percent.

(b) The average absolute efficiency value of the known series of the first and second sets of experiments exceeds that of the corresponding unknown by 10 per cent.

(c) The average absolute efficiency value of the known series of the third set of experiments exceeds that of the corresponding unknown by 10 per cent. (The ergograph load in this set is 6 kilograms.)

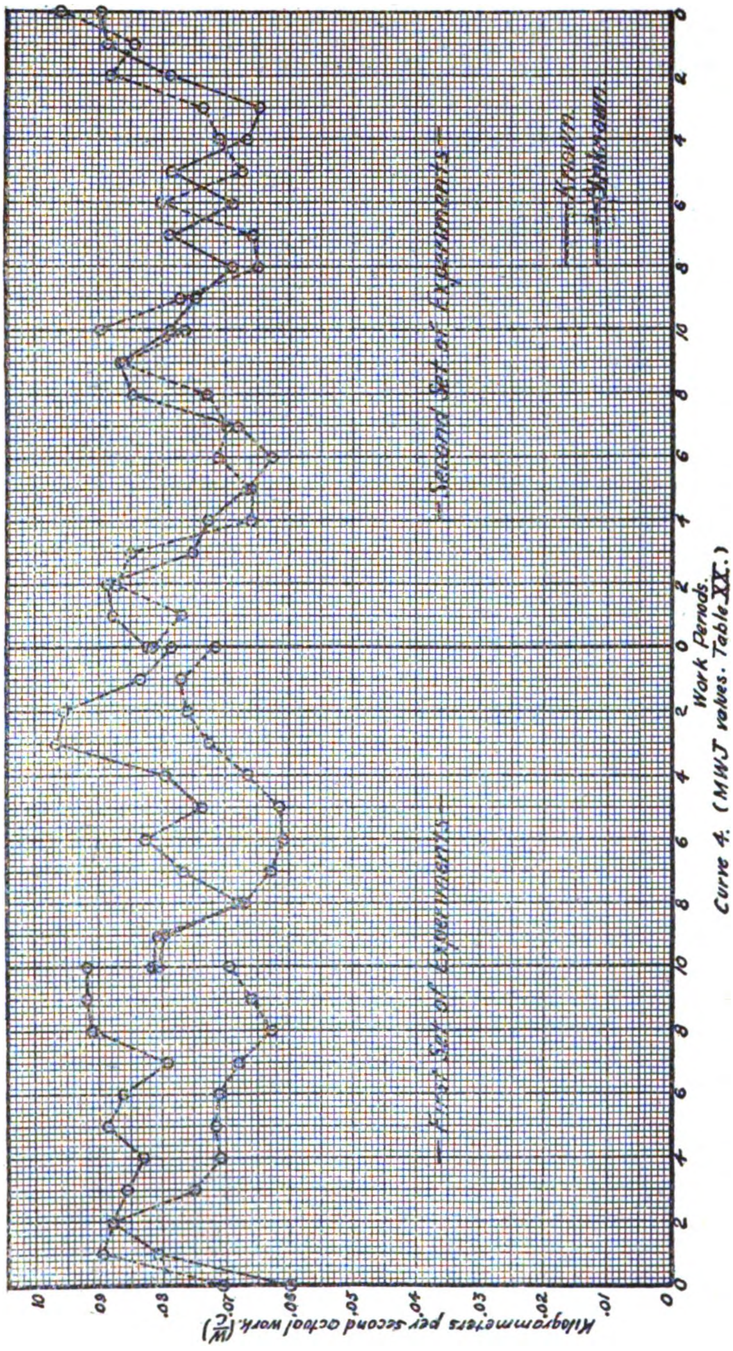
(d) The average unit value of the known series of all sets of experiments shows a higher rate of work by 18 per cent over the corresponding value of the unknown series.

(e) The average unit value of the known series of the first and second sets of experiments shows a higher rate of work (speed) by 5 per cent over its corresponding value of the unknown series.

(f) The average unit value of the known series of the third set of experiments shows a higher rate of work by 35 per cent over the corresponding value of the unknown series.

A comparison of "a" and "b" indicates that the average absolute efficiency value is independent of the ergographic load. The known series is 10 per cent more efficient whether the load is four or six kilograms.

Reference to "e" and "f" shows that the average rate of work is dependent upon the load. Here we find that the average unit value of the known series exceeds that of the unknown by 5 per cent when the ergographic load is 4 kilograms, while the unit value under similar conditions for the known series exceeds by 35 per cent the unknown when the load is increased by 2 kilograms. Within the limits here investigated the rate of work increases directly as the load increases. An increase by one-half of



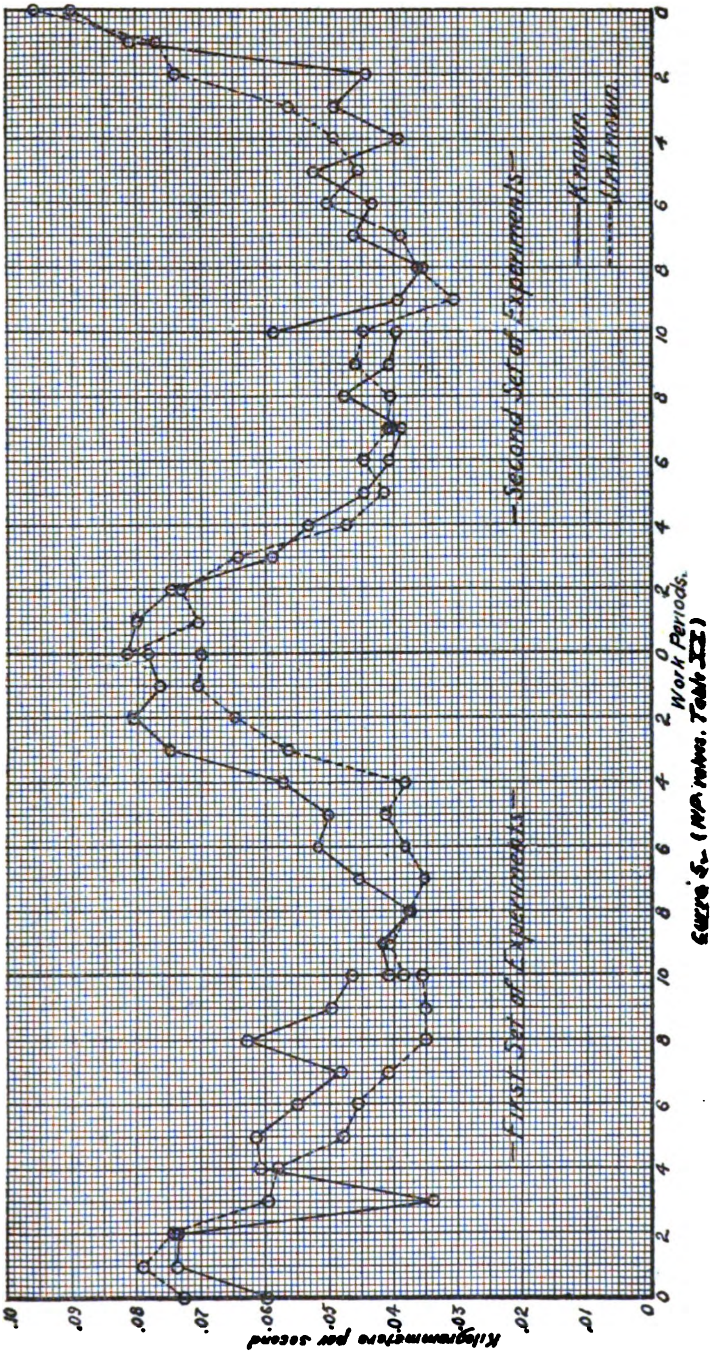
Curve 4. (MWJ values. Table XX.)

TABLE XX

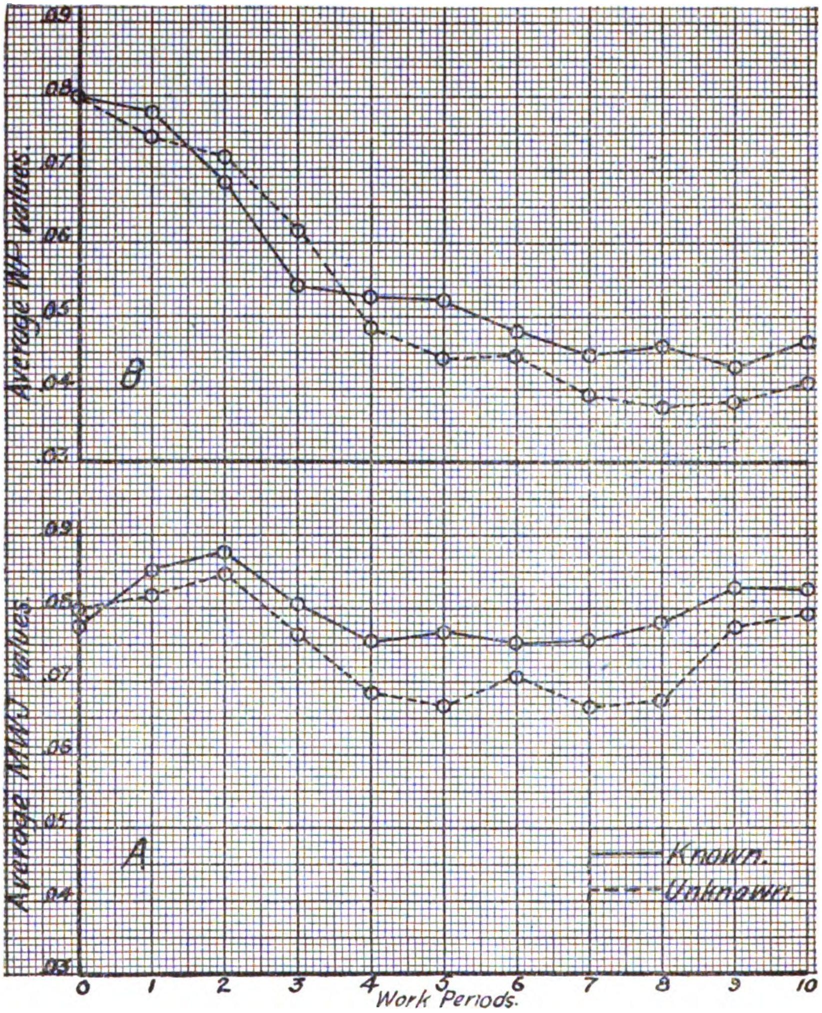
Summary of Tables I to IX. With Respect to "W.P." and "M W J" Unit Values $(\frac{W}{C})$

Known Work Periods →	0	1	2	3	4	5	6	7	8	9	10
WP $\frac{\text{KgM.}}{\text{Sec.}}$ 1st Asc...	.0595	.0737	.0735	.0338	.0605	.0614	.0549	.0481	.0627	.0497	.0465
" " 1st Desc..	.0782	.0763	.0803	.0748	.0572	.0500	.0518	.0453	.0377	.0418	.0404
" " 2nd Asc..	.0816	.0800	.0747	.0586	.0532	.0446	.0404	.0383	.0474	.0409	.0397
" " 2nd Desc.	.0898	.0809	.0443	.0493	.0392	.0524	.0431	.0460	.0355	.0393	.0585
Average	.0795<	.0777	.0682<	.0541<	.0525	.0521	.0478	.0445	.0458	.0429	.0463
Unknown Work Periods											
WP $\frac{\text{KgM.}}{\text{Sec.}}$ 1st. Asc...	.0725	.0788	.0739	.0595	.0579	.0479	.0454	.0407	.0350	.0350	.0354
" " 1st Desc..	.0700	.0704	.0648	.0564	.0382	.0414	.0383	.0351	.0373	.0409	.0383
" " 2nd Asc..	.0817	.0704	.0731	.0641	.0473	.0414	.0446	.0410	.0404	.0459	.0448
" " 2nd Desc.	.0957	.0767	.0738	.0564	.0493	.0456	.0502	.0387	.0361	.0305	.0449
Average	.0797	.0741	.0714	.0616	.0482	.0441	.0446	.0388	.0372	.0381	.0408
Known Work Periods											
MWJ $\frac{\text{KgM.}}{\text{Sec.}}$ 1st Asc..	.0595	.0805	.0878	.0854	.0828	.0883	.0859	.0789	.0908	.0917	.0917
" " 1st Desc.	.0783	.0832	.0951	.0963	.0793	.0735	.0824	.0761	.0675	.0793	.0804
" " 2nd Asc.	.0820	.0875	.0883	.0750	.0726	.0663	.0625	.0679	.0844	.0864	.0785
" " 2nd Desc.	.0895	.0886	.0786	.0648	.0664	.0783	.0687	.0785	.0688	.0745	.0788
Average	.0773<	.0850	.0875	.0804	.0753	.0766	.0750	.0754	.0779	.0829	.0824
Unknown Work Periods											
MWJ $\frac{\text{KgM.}}{\text{Sec.}}$ 1st Asc..	.0705	.0893	.0876	.0747	.0707	.0714	.0708	.0677	.0624	.0658	.0692
" " 1st Desc.	.0713	.0768	.0759	.0723	.0662	.0612	.0608	.0627	.0669	.0803	.0814
" " 2nd Asc.	.0814	.0769	.0873	.0847	.0657	.0663	.0710	.0696	.0726	.0864	.0761
" " 2nd Desc.	.0957	.0842	.0879	.0736	.0710	.0672	.0799	.0656	.0648	.0768	.0896
Average	.0797	.0818	.0847	.0762	.0684	.0665	.0706	.0664	.0672	.0773	.0791
Av. Diff. between Kn. and Unkn. WP's	.0002<	*.0036>	.0032<	.0075<	.0043>	.0080>	.0032>	.0057>	.0086>	.0048>	.0055>
Av. Diff. between Kn. & Unkn. MWJ's	.0024<	.0032>	.0028>	.0042>	.0069>	.0101>	.0044>	.0090>	.0107>	.0056>	.0033>

* Sign > or < means that the known value is greater or less than the corresponding unknown value.



Curve 5. (10 mins. Table XX)



Curve 6. (Table XX)

TABLE XXI

Summary of Tables I to IX and XII to XIX inc. With Respect to Absolute Values (W).

Known Work Periods	0	1	2	3	4	5	6	7	8	9	10
Absolute values	6.66	8.29	9.32	9.10	19.51	22.84	21.79	24.57	33.93	55.68	54.77
"	11.18	15.45	20.07	28.43	79.86	98.00	48.80	73.56	79.12	66.87	97.36
"	11.64	14.88	20.45	21.52	29.78	33.85	64.94	92.69	153.45	199.03	244.64
"	11.00	12.88	14.15	27.44	34.92	52.59	69.46	67.55	109.90	161.56	233.23
"	5.96	9.68	7.92	12.72	13.04	20.32	13.40	22.44	24.96	38.17	42.32
"	4.44	3.98	8.08	17.50	10.61	15.75	48.00	45.85	68.93	73.14	104.47
"	7.69	12.86	21.50	9.74	33.08	16.89	31.57	71.69	73.87	84.67	91.02
"	6.12	7.92	9.64	8.12	13.80	12.48	21.16	24.84	36.04	27.60	43.28
"	6.90	7.06	16.01	38.31	39.22	53.60	58.12	52.78	61.17	60.58	68.70
"	9.29	13.08	5.89	12.39	18.88	40.53	65.79	70.03	98.54	131.27	143.95
"	9.71	16.65	9.33	24.28	16.93	24.40	48.89	40.78	78.38	101.67	144.78
Average	8.23	11.16	12.94	19.05	28.15	35.57	44.72	53.34	74.39	90.20	115.04
Unknown Work Periods											
Absolute values	8.65	10.57	11.63	12.40	17.53	22.94	17.36	26.98	39.12	78.11	70.34
"	10.64	17.36	15.70	19.80	25.05	36.64	35.96	66.24	65.19	64.29	111.37
"	12.00	16.36	13.87	26.04	23.70	55.34	59.24	59.52	140.39	161.54	226.34
"	11.06	15.48	20.54	21.42	52.63	57.75	100.81	107.11	96.58	133.39	132.06
"	6.00	8.60	12.09	14.45	13.44	22.94	29.13	24.54	39.60	47.88	54.75
"	6.10	7.48	5.87	11.78	6.97	10.66	33.75	25.91	54.65	99.65	66.40
"	7.27	7.24	11.19	22.46	14.96	54.77	30.71	56.53	49.57	87.23	86.70
"	5.68	8.84	8.12	11.06	8.60	11.88	16.84	31.36	21.68	27.68	32.96
"	4.18	10.11	8.35	18.14	11.75	31.94	41.56	54.31	38.81	38.03	72.34
"	9.28	6.62	14.80	10.96	28.41	34.47	66.98	63.30	53.40	76.73	86.38
"	10.65	15.94	7.78	14.83	28.53	36.25	48.23	63.33	70.39	130.65	133.24
Average	8.32	11.33	11.81	16.66	21.05	34.14	43.69	52.65	60.85	85.92	97.53
Difference between Av. Kn. & Unkn. Absolute Values (W) and (>) greater or (<) less than unkn. values	.09 KgM. <	.17 KgM. <	1.13 KgM. >	2.39 KgM. >	7.10 KgM. >	1.43 KgM. >	.03 KgM. >	.69 KgM. >	13.54 KgM. >	4.28 KgM. >	17.51 KgM. >

TABLE XXII

Absolute (W) and Unit ($\frac{W}{c}$) Values	All Observers Average W for Table I-VIII and XIII-XIX Inc. Kg M.	Average $\frac{W}{c}$ for Tables I-VIII and XIII-XIX Inc. $\frac{\text{Kg M.}}{\text{Sec.}}$	Relation of Av. Known of WP (Table XX) to Av. Unknown WP (Table XX)	Relation of Av. Known MWJ (Table XX) to Av. Unknown MWJ values (Table XX)	Observer M Average W for Tables IX-XII Inc. Kg M.	Average W for Tables I-VIII Inc. Kg M.	Average $\frac{W}{c}$ for Tables I-VIII Inc. $\frac{\text{Kg M.}}{\text{Sec.}}$	Average $\frac{W}{c}$ for Tables IX-XII Inc. $\frac{\text{Kg M.}}{\text{Sec.}}$	Average W for Tables I-XII Inc. Kg M.	Average $\frac{W}{c}$ for Tables I-XII Inc. $\frac{\text{Kg M.}}{\text{Sec.}}$
Known Series	44.79	.0796	.0546 : .0526 $\frac{\text{Kg M.(1)}}{\text{Sec.}}$	.0796 : .0743 $\frac{\text{Kg M.(2)}}{\text{Sec.}}$	13.70	59.05	.0870	.2127	49.33	.1139
Unknown Series	43.95	.0743			12.30	53.05	.0824	.1347	44.36	.0936
Percent	.2	7-			10	10	5	35	10	18

(1) Amount of work done per second inclusive of the rest seconds.
(2) Amount of work done per second exclusive of the rest seconds.

over .0345), since the zero work period of this series follows the tenth period of the known series. The corresponding values for observers W and J are .096 and .045 for the known and .016 and .038 for the unknown work periods respectively (Tables XIII, XVIII, XVII; curves 2 and 3). The enormous efficiency of observer W for the eighth work period of the first ascending known series and the low efficiency for the corresponding period of the unknown series remain unexplained.

The composite curves, 4 and 5 comprise the unit values found for the three observers. Curve 4 shows the relation of the unit values of the known and unknown work periods of all the series for all observers. That part of the curve which shows the relation of the unit efficiency values of the known and unknown work periods for the first set of experiments clearly indicates that "knowledge of results" is a favorable condition for work, for in this set, only four unknown work periods of the total number of periods (22 known and 22 unknown) exceed in efficiency their corresponding known periods.

The gradual appearance of the determining presence of imagery as a favorable condition in efficiency is evidenced by the fact that in the second set of experiments ten unknown work periods exceed in efficiency the known, while in the first set, as already stated, the unknown excelled the efficiency of the known work periods in four of the total periods of the set. Two and one-half times as many unknown work periods excel the known in the second set of experiments as in the first set. In two of the periods of this set the efficiency values of the known and unknown work periods are equal. These results when considered in conjunction with certain introspections given below, indicate that representative conscious factors in the unknown work periods of the second set of experiments possess the same *raison d'être* as the immediate conscious content present in the known periods of the same set.

In Curve 5, the seconds of rest are included in the efficiency values per unit of time. The character of the curve remains essentially the same as in Curve 4—practically unmodified by the inclusion of the rest seconds.

the original load is followed by a corresponding increase in the rate of work seven times that of the original rate. That is, the loads are to each other as 2:3 while the corresponding rates of work are as 1:7.

Consideration of Curves

The six curves set forth the relationship of the unit values of the known work periods to the corresponding values of the unknown periods.

Curves 1, 2, and 3 show the relative individual unit efficiencies of each observer. Each of these curves is really made up of eight curves corresponding to the unit values of four known and four unknown series of work periods, excepting curve 3 which is made up of the efficiency unit values of three known and three unknown series. These curves show general agreement in that in the first set of experiments when the observer is aware of his progress he is strikingly more efficient than when he is ignorant of his progress. They also show that this difference is gradually overcome so that in the second set the advantage of the known series is not so apparent.

The general superiority of the efficiency values of the known work periods over that of the unknown is unequivocal for the various series of the first set of experiments. This is not true of the first two series of the second set of experiments as the three individual curves clearly show; it is still less true of the last two series of the second set. This interesting phenomenon is concomitant *with the appearance of imagery which seemingly parallels in function certain features of the mental complex operative for the known work periods.* A more complete discussion follows below.

The maximum advantage of a known work period over that of an unknown, in the case of observer M, is .0345 kilogrammeter seconds of work (Tables III and IV—4th work period, 4 seconds of rest); the maximum advantage of an unknown work period over that of a known is .0439 kilogrammeter seconds of work (Tables I and II). Obviously practice effects account for the superiority of the latter difference over the former (.0439

Curve 6 shows the average efficiency values per unit of time for all known work periods of all observers as compared to the corresponding unknown periods. Of curve 6, A shows the actual amount of work performed per second, i.e. at what rate the load was lifted. Part B of the curve shows the rate of work when the rest seconds are included. In one period (zero) only does the actual rate of work of the unknown period exceed that of the known. From A it appears that short rest periods of between one and three seconds are the most favorable and that medium rest periods of between four and eight seconds are the least favorable for maximum efficiency.

The Appearance of Imagery

The relation of the unit values of the known and unknown second ascending series of the second set of experiments is especially interesting in that the phenomenon of overlapping of the curves is concomitant with the appearance of imagery. From the introspections it appears that the condition of "ignorance of results" prevailed more completely in the first set of experiments than in the second set.

The approach of the values of the work periods of the unknown series of the second set of experiments to the values of the work periods of the corresponding known series is concomitant with the appearance of visual and kinaesthetic imagery. This imagery is reproductive of the perceptual experiences of the known period of the preceding series.

In the third work period of the second set of experiments, (Curve 1), the following introspection is recorded: "The task of this period proceeded with a fair degree of definiteness, with a comfortable degree of orientation hardly comparable to any of the preceding unknown periods. Certain individual lifts I pictured vividly; in certain other cases I compared successive lifts. The comparisons were especially pronounced when the first evidence of fatigue appeared." In the sixth period of the same series the observer remarked: "I seemed to sense the efficiency of the entire period in perspective more or less tangibly."

Imagery is also involved in the more or less abrupt close of the unknown period as contrasted with the gradual tapering off close of the known periods. On this point one of the observers remarks as follows: "In closing an unknown period I seem to let down suddenly in spite of all efforts to avoid it. Short lifts have little meaning in that I fail to image the pen marks which appear to support me in the long lifts."

In all these cases evidently the presence of imagery of the sort here described indicates that an effective condition for work prevailed. The presence of imagery was first detected in the first work period of the second unknown ascending series of the second set of experiments. It is interesting, as already noted, that the overlapping of the curves of this set of experiments is concomitant with the presence of imagery. As the imagery content of consciousness peculiar to the work periods of the unknown series approximates that of the perceptual content characteristic of the work periods of the known series, the differences in the awareness of results diminish. It is probable that the more essential features of the perceptual experiences acquired in the first set of experiments function in imagination in the work periods of the unknown series of the second set of experiments. If the mental complex present in the second set of experiments forms a close resemblance to that complex present in the first set, and, if the efficiency differences of the known and unknown work periods are primarily due to conditions indicated by the degree of "knowledge of results," then we have a clue for the crossing back and forth of those parts of the curves representing the unit values of the work periods of the second set of experiments (Curves 1, 2, 3, 4, 5).

The question may well arise as to why the values of the work periods of the known series do not approach those of the corresponding unknown series. Such an approximation would clearly be contrary to the law of habit. In general the normal work response is essentially perceptual in character; the particular ergographic work period in which the observer is aware of the details of his progress conforms to the normal procedure. The unknown work periods deviate from the normal procedure and are therefore, less effective. The normal procedure involves a high degree of adaptation and, therefore, insists, as it were, that all allied deviates shall conform to its general mode of operation. This may explain why the values of the unknown work periods of the second set of experiments approximate those of the corresponding known values. The effects of the fully perceptual (known) procedure tends to drain the relatively non-perceptual (unknown) just in proportion to the extent to which the former have become habituated. Moreover, the perceptual response is normally more compelling and effective because more direct.

In the above event it appears that the presence of imagery is a mere indication that the non-perceptual has taken on the character of the perceptual procedure and this because the latter is the dominating mode of behavior.

From the general character of these curves and the introspective statements it appears that work carried on under conditions of partial awareness of results loses in efficiency and that such conditions are difficult, if not impossible, to maintain when followed or preceded by work of identical character under conditions of complete awareness. Without set purpose the observers experience certain mental complexes during work under the former conditions, which parallel certain essential features of the mental complexes present during work under the latter conditions.

Ergographic Records

A total of two hundred sixty-six records were taken of which nine typical ones are here reproduced. Five of these, (II, III, V, VIII, IX) record the efficiency of the observer when working under conditions of complete awareness of his accomplishment; the remaining four, (I, IV, VI, VII), likewise record efficiency but under conditions of relatively complete ignorance of results.

Seven of the nine records are given complete; Records VIII and IX have 164 and 230 subdivisions cut off respectively. The cut off subdivisions could serve no purpose here which is not already attained by the tabulated data. The reproduced parts of records VIII and IX, reveal what is important for our purpose here, viz., the characteristic close of work periods of extremely long duration. For the former record (See Table VII) the observer was in the machine 1 hour, 3 minutes and 49 seconds; for the latter, 1 hour, 27 minutes and 30 seconds.

(*a*) *The Phenomenon of 'Sudden Recoveries'*

An examination of these records, particularly records VIII and IX, shows the very interesting and curious phenomenon of sudden and unexpected recoveries. In the subdivisions representing sudden recoveries the lifts were nearly as high as the lifts which were made in the earliest subdivision of the period. Reference to record VIII shows that three such recoveries are especially prominent: (1) A, including subdivisions 180 and 181; (2) B, subdivisions 186 to 192 inclusive; and (3) C, subdivision 196. The same phenomenon is probably found in record I at lifts marked "a" and "b"; in record II at "x" and "y." It is not improbable that a definite ratio exists between the frequency and extent of these sudden increases in efficiency and the duration of the work period.

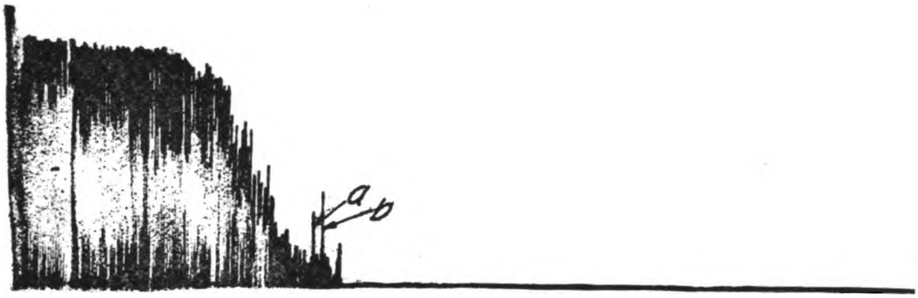
It should be recalled at this point that the observer was instructed before the beginning of each work period and occasionally reminded during the progress of the period that each lift must represent maximal effort. It can not be maintained therefore that the subdivisions following 181 and preceding 186, for example, afforded an opportunity for rest as their efficiency values would seem to indicate. In fact, these subdivisions (182-185 inc.) are among the most laborious of the entire period. The statements below make it difficult to explain the phenomenon in such easy fashion as 'increase of attention to the work' or 'mechanical imperfection in the apparatus.' The phenomenon is found frequently in periods of very long duration and for all observers, but it is not equally prominent for all observers nor in all the periods of a given observer (Record VIII and IX). Schenk[7] seems to have obtained an aberrant fatigue curve when the work period covered one hour. It is doubtful

---

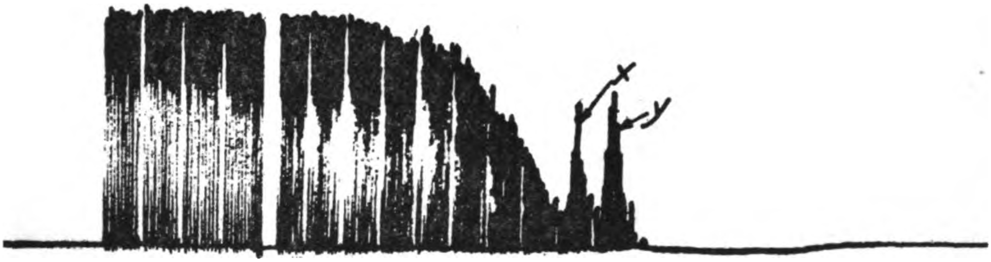
[7] Archiv. f. d. ges. Physiologie, 1900.

[8] Amer. Jour. of Physiology, Vol. V, p. 248.

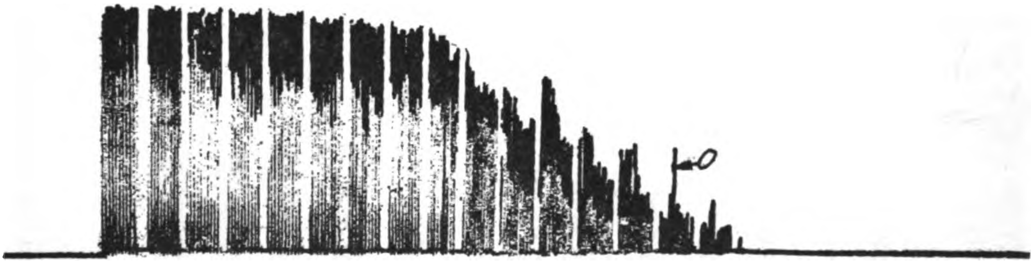
[9] Amer. Jour. of Psychology, Vol. III, pp. 25, 26.



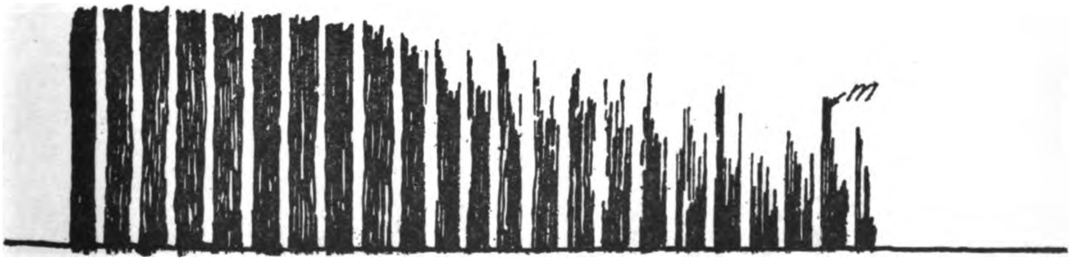
Record I. Unknown Series. No Rests. $\frac{W}{C} = .087 \frac{KgM.}{Sec.}$



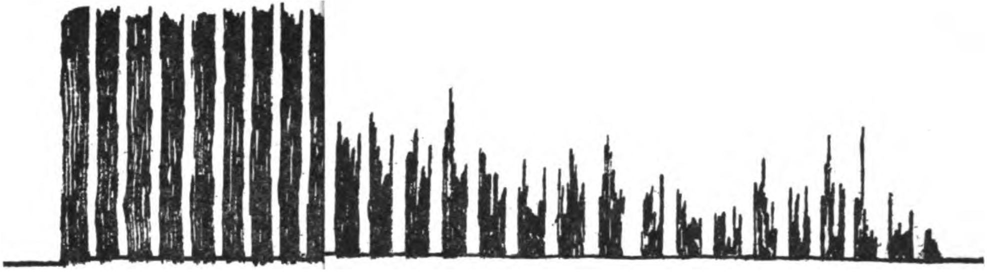
Record II. Known Series. 1" Rest to 10" Work. $\frac{W}{C} = .091 \frac{KgM.}{Sec.}$



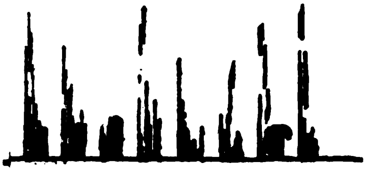
Record III. Known Series. 2" Rest to 10" Work. $\frac{W}{C} = .087 \frac{KgM.}{Sec.}$

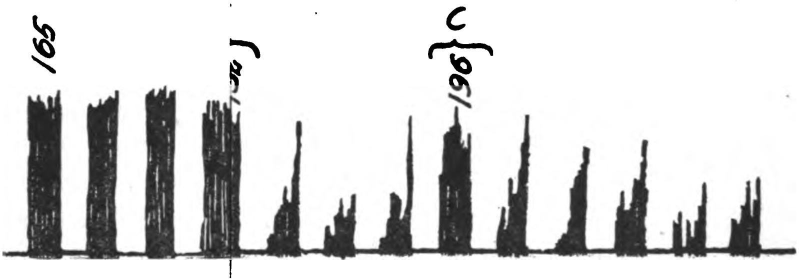


Record IV. Unknown Series: 3" Rest to 10" Work. $\frac{W}{C} = .080 \frac{KgM}{Sec.}$

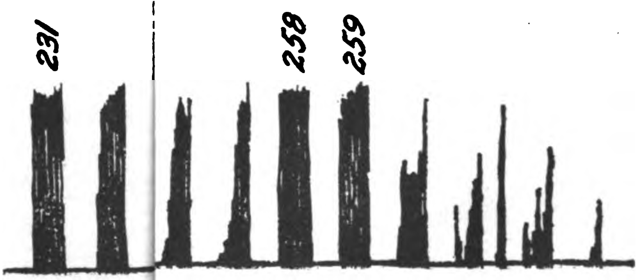


Record V. Known 10" Work. $\frac{W}{C}$=.070 $\frac{KgM.}{Sec.}$





Record VIII.



Recor

whether fatigue curves of short or long duration are ideally asymptotic as Hough[8] maintains.

The work of Lombard[9] has so direct a bearing upon the 'phenomenon of sudden recovery' that I take occasion to quote him at length. He lifted a weight of three kilograms by the flexor muscles of the second finger of the left hand every two seconds until exhausted. "Each contraction was the strongest possible" and "I was determined to fatigue the muscles so completely that no contraction should be possible. After 110 seconds of continuous work, I could *hardly stir* (italics mine) the weight, and thought the experiment nearly at an end. *To my surprise* (italics mine), however, I began to recover the lost power, and during the next half minute each of the succeeding contractions was higher than the one which had preceded it. The effect of fatigue then began to manifest itself again, and the contractions became smaller. I concluded that I had made a mistake, that I had not exerted all my will power before, and I determined for the rest of the experiment to do my best. As the contractions grew smaller I threw all my power and energy into each attempt to raise the weight. I was conscious . . . that my face was flushing under the strain; nevertheless, the contractions became gradually less, and I supposed that I had finally succeeded in tiring out the muscles, when, *to my astonishment* (italics mine), I began to recover my power a second time. The contractions became stronger, reached a maximum and fell, only again to recover. In short, for some inexplicable reason . . . the ability to voluntarily contract the muscles with sufficient strength to raise the weight, decreased and recovered five times." These statements are interesting and take on added significance when compared with the statements of observer M given below.

Lombard[10] believes the phenomenon constant and normal and that it appears "after considerable work has been performed."

With regard to these sudden and unexpected recoveries Observer M remarks as follows: "During subdivisions 182 to 185 inclusive I experienced a sharp pain unlike anything previously experienced. It disappeared quite suddenly at the beginning of

---

[10] Ibid., p. 30.

186th subdivision. For the next six subdivisions my experience was most unusual. My finger seemed detached and lifting of its own accord, apparently without any effort on my part and with an indescribable sense of exhilaration. I seemed to be beginning all over again except that I seemed to have no part in lifting. The recovery was sudden, unexpected and as though something had been cut loose. At these times the lifts were uncontrolled while I seemed nothing more than an interested spectator."

The phenomenon here described is not confined to records VIII and IX but is observable in most of the long and many of the short records, as the introspections show. For subdivision 258, Record IX the following introspection is recorded: "I seem to begin all over again except that the feeling of effort incidental to initial action is lacking and I enjoy a peculiar sense of ease and agreeableness. Strain and tension for the time being disappear while the muscular mechanism functions almost automatically. The conscious correlate so far as it may be considered "causal," subsides almost to the vanishing point. At these times the 'will' seems to have very little to do with the lifting. This is interesting in view of the fact that the periods, especially those of long duration, are extremely disagreeable due to the condition of the experiment, viz., that each lift must represent maximal voluntary effort. The attentive strain is enormous."

(*b*) *Interpretation of 'Sudden Recoveries'*

There is no accounting for this phenomenon on the ground of greater determination or a stiffening resolution, for each lift, according to the conditions of the experiment, represents maximal effort. A plausible explanation may be found in the assumption that certain efferent neurones are serially related to particular muscles or groups of muscles. Within a group we may assume a serial arrangement of neurones of constantly diminishing functional efficiency. The number of neurones in the group, their order of efficiency and their functional appearance are probably indicated by the recovery periods on the ergographic records. It seems that action over a given set of neurones does not exhaust the energy of the muscles with which the neurones are connected but that continued innervation by means of these neurones is followed by increasing impermeability of the motor end-organ. If now the cerebral line of discharge takes place over a different set of neurones the link between neurone and motor end-organ again becomes permeable. Points marked "A," "B" and "C" on Record VIII may be regarded as reestablished points of permeability. Following this hypothesis exhaustion appears when the junction between the cerebro-muscular chain can not be maintained.

Whether impermeability is a result of cerebral or peripheral fatigue or both is a question not yet finally settled.[11] Lombard,[12] it may be mentioned, conducted a series of experiments which lead him to believe that the sudden recoveries (periodic variations) are of "central rather than peripheral origin." With reference to these experiments he says: "These and other similar experiments convinced me that the changes producing the periodicity, do not occur in the nerves, the nerve ends, in the muscles, and that they take place in the central nervous system." Moreover, he believes that the phenomenon of periodicity is distinctly connected with voluntary contractions since periodicity (sudden recoveries) fails to appear when the muscles are electrically stimulated.

McBride[13] seems to confirm Lombard in that central fatigue is the important factor which finally terminates the work. It seems to be quite generally agreed that the point of voluntary inefficiency is only secondarily related to peripheral fatigue.

Weichardt[14] holds that the body in the course of its diurnal activity developes an antitoxin against fatigue-products which are rendered, at least partially, ineffective. If such toxins are actually present we may find a plausible explanation for the phenomenon of 'sudden recoveries' (A, B, C, Record VIII) in the periodic rise in efficiency of the toxins.

Our fatigue records (I to IX, or any fatigue curves) may then be regarded as to some extent records of the interaction of the two kinds of toxins. These must be regarded with respect to function as antagonistically related. In these circumstances the fatigue-toxins and their antitoxins are negatively and positively related to muscle respectively. Work periods 249, 250, 258, 259 in Record IX represent periods of special effectiveness of the antitoxins. Such periods are again indicated by "a" and "b," in Record I; "x" and "y" in Record II; "o" in Record III; "m" in Record IV.

---

[11] Storey, T. A., in the Amer. Jour. of Physiol., Vol. 8, p. 373 and Lee and Everingham in Am. Jour. of Physiol., Vol. 24, p. 384 give the important literature concerning the seat of fatigue.

Abelous, J. E., Contribution a l Etude de la Fatigue, Arch. de Phys. 1893, 3.

[12] Ibid., pp. 30-35.

[13] Journal of Nerv. and Mental Diseases, 1901, p. 628.

[14] Mun. Med. Wochenschrift, 1904 and Weber Ermudüngstoffe, 1910.

That the phenomenon of periodicity (recoveries) is one not exclusively connected with voluntary contraction seems conclusively established by Storey[15] and others. Instead of voluntarily stimulating human muscle he employed graded increases in the strength of an induction current. The subdivision intervening between 173 and 180; 181 and 186 and between 192 and 196 in Record VIII may be regarded as constituting Storey's "intervals of lower contractions." This phenomenon had previously been observed by a number of German investigators[16] in experimenting with muscle-nerve preparation from the frog.

SUMMARY

1. Within the limits operative for the present study both the absolute amount of work and the rate of work done under conditions of knowledge of results exceed that done under conditions of ignorance of results. The two sets of conditions do not maintain the same degree of discreteness throughout the entire duration of the experiment. As the latter conditions, indicated by reproductive imagination (see "3" below), approximate the former conditions, the relation of the work value becomes ambiguous. (Curve 1, second set of experiments).

Will power as conventionally regarded is inadequate to explain the efficiency differences. It is more likely that the neuro-muscular chain underlying the lifting response functions more efficiently when the afferent channels from the eye are open than when they are closed. The former condition appears provocative of greater functional changes in the central nervous system than the latter, which changes operate to maintain attention and increase muscular efficiency.

---

[15] Am. Jour. of Physiol. pp. 439, 440. The important literature on the phenomenon in question is cited in this reference and need not be reproduced here.

[16] Ibid.

Attention may well be a "disinterested spectator" and a mere sign for the degree of central functional changes which become determinants of the efficiencies of the various work periods. Habituation to maximum voluntary effort under the two conditions of the experiment would probably reduce to a minimum the efficiency difference apparent for the two sets of conditions. In other words, efficiency differences probably represent degrees of the process of habituation.

2. When work is long continued and to the point of exhaustion a curious phenomenon of sudden recovery appears which can not be entirely identified with phenomena peculiar to fatigue. This phenomenon is especially pronounced in work periods of extended duration. Such sudden fluctuations in efficiency find a plausible explanation in a functional grouping of the neurones connected with the group of muscles involved in lifting the ergographic load.

3. The condition of 'ignorance of results' is more effective in the first set of experiments than in the second set. In the latter set imaginative elements, in the unknown series of experiments, parallel the perceptual elements in the known series of the same set. The ambiguity of the efficiency values obtained under the two sets of conditions for the second set of experiments may be regarded as an interesting stage in the process of habituation.

4. An increase of the ergographic load by 2 kilograms decreases the variations in the amount of work done in successive work periods and, such increase does not essentially affect the conclusions drawn from the first and second sets of experiments.

5. The optimal rate of work under the conditions of the experiment lies between .075 and .085 $\frac{\text{Kg M.}}{\text{Sec.}}$

6. The commonly observed staircase (Bowditch "treppe") contractions appear irregularly and for the most part in the closing subdivisions of a work period. This is probably due to the fact that each lift represents maximal contraction and that each lift is negotiated by muscles thoroughly trained.

GENERAL LIBRARY
JUL 22 1920
UNIV. OF MICH.

Vol. XXVIII
No. 4

PSYCHOLOGICAL REVIEW PUBLICATIONS

Whole No. 126
1920

THE
Psychological Monographs

EDITED BY
JAMES ROWLAND ANGELL, 522 FIFTH AVE., NEW YORK.
HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*J. of Exp. Psychol.*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*) and
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

Individual Differences in Finger Reactions

By

ESTHER L. GATEWOOD
Carnegie Institute of Technology
Pittsburgh, Pa.

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.)
PARIS (16 Rue de Condé)

Psychological Review Publications

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*J. of Exp. Psych.*)
JAMES R. ANGELL, UNIVERSITY OF CHICAGO (*Monographs*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*)
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews, notices of books and articles, psychological news and notes, university notices, and announcements, appears monthly, the annual volume comprising about 480 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 480 pages. (Suspended temporarily.)

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in May, and may be subscribed for in connection with the periodicals above, or purchased separately.

ANNUAL SUBSCRIPTION RATES

Review and Bulletin: $6. (foreign, $6.50). **Journal:** $3.25 (foreign, $3.50).
Review and Journal: $6. (foreign, $6.50). **Bulletin:** $3.25 (foreign, $3.50).
Journal and Bulletin: $6. (foreign, $6.50).
Review, Bulletin and Journal: $9. (foreign, $9.75).
Index with any other two: $7. (foreign, $7.50).
Review, Bulletin, Journal and Index: $10. (foreign, $10.75).

Current Numbers: Review, 65c; Bulletin, 35c; Journal, 65c; Index, $1.25.

THE PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. THE MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages, with a uniform subscription price of $5.50. (Postal Union, $5.80.)

Philosophical Monographs: a series of treatises more philosophical in character.
Library of Genetic Science and Philosophy: a series of bound volumes.

Subscriptions, orders, and business communications may be sent direct to the

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, NEW JERSEY

FOREIGN AGENTS: G. E. Stechert & Co., London (2 Star Yard, Cary St., W. C.);
Paris (16, rue de Condé)

INDIVIDUAL DIFFERENCES IN FINGER REACTIONS

INTRODUCTION

The purpose of this experiment has been to investigate the speed and accuracy of the reactions of the fingers of the two hands in response to visual stimuli. Many suggestions have been put forth from time to time, offering explanations for the lack of regularity between the action of the various fingers when applied to piano playing. These are for the most part mere observations, or at best unscientific reports. A little volume by Grabill[1] and one by Eckhardt[2] are two of the few studies that really attack the fundamentals underlying control and adjustment of the members used in piano playing.

Since piano playing is to a large degree dependent on the proper control of speed, energy and accuracy in finger movement, it seemed important to investigate the relationship between time and accuracy, leaving the more involved factor of energy to subsequent experimentation. Piano teachers have been content to go blindly on, using the hit-and-miss method, or where there has been developed a more or less successful method, it has never been reduced to such objective terms that the teacher may say to the student, "We know this because it has been experimentally demonstrated."

If one can experimentally show that some fingers are inherently slower and less accurate in their responses than others, then exercises for the development of particular fingers may be devised and used to advantage. If these fingers be the same for each individual, the process is simpler. However, if these fingers be found different according to the individual, the method may still be employed, providing the individual differences can be determined before the student begins his work.

---

[1] Grabill, E. W., "The Mechanics of Piano Technique, " Chicago, 1909.

[2] Eckhardt, Robt., "Piano Exercises for the Development of Side-Finger Action and the Development of an Equal and Maximum Spread between the Fingers," Columbus, 1899.

It seems desirous to investigate not only the individual finger differences, but also the various combinations and reactions that involve more than one finger. Briefly stated, the investigations reported in this paper fall under the following heads:

1. The relation between speed and accuracy of the individual fingers.
2. The relation between speed and accuracy of the various finger combinations, both when responding with one finger at a time and with two at a time.
3. The relation between the speed and accuracy of the left and right hands.
4. The relation in speed and accuracy between two-finger and one-finger responses.
5. The relation in speed and accuracy between two-hand and one-hand reactions.
6. The differences between individuals on the basis of speed and accuracy.
7. The effect of practise.

HISTORICAL

The history of the study of reaction times dates back to the time when the astronomers first attempted to measure the personal equation in transit obervations. Sanford[3] records that Maskelyne, the British Astronomer Royal, first noticed that his records differed from those of his assistant by 1/2 second. The astronomer reported that the assistant had been in this error for some time before he noticed it and that he did not seem likely to get over it. He supposed that the assistant had fallen into some irregular and confused method of observation instead of using the "excellent (Bradley) method." The assistant was accordingly discharged. For twenty years after this incident no mention was made of individual differences. In 1816 von Lineman mentioned it and then it fell under the eye of Bessel, who in 1822 published the results of the first tests that showed individual differences in recording the times of steller transits.

---

[3] Sanford, E. C., "Personal Equation," Am. Jour. of Psychology, v. 2, pp. 1-38.

All of this early work was done by the "eye and ear" method. When the star is about to make its transit, the observer reads off from his clock, and then he watches the star in the telescope, and continues to count the second beats. He fixes firmly in mind, as the moving image approaches the wire, its place at the last beat before it crosses the wire and its place at the first beat after. From the distances of these two points from the wire, he estimates by eye the time when the star crossed in tenths of a second. "The role of the mind in observations by this method," says Sanford, "is the fixing of the exact place of the star at the first beat, the holding of the same in memory, the fixing of the place at the second beat, the comparison of the two, and the expression of the relation in tenths." It was not until 1850 that the chronograph was first used to record the time at which a star crossed the meridian. This is the first recorded example of reacting with a key to a visual stimulus.

In the same year (1850) Helmholtz published the results of the first simple reaction time experiments. This was the beginning of the psychological period and the purpose of the most of these experiments was to determine the speed of nervous conduction. After determining the speed of conduction in the motor nerve of the frog to be 27 meters per second, Helmholtz measured the physiological time (in man) which elapsed between the stimulation by a weak stimulus and the execution of the movement. He found the total time ranged from 125 to 200 sigmas, the first reaction times published. This was followed by experiments on the part of many investigators, each trying to establish the rate of conduction of the nervous impulse. The results of most of the experiments differed and Du Bois Reymond in 1900 showed that the differences found were all probably due to too small numbers of subjects and trials and to faulty technique.

The first experimental work done from a psychological interest was that done by Donders and deJaager, published in

1865. Henmon[4] classifies the various phases of the problem of reaction time into three groups:

1. Time relations of simple and complex mental processes and their variations with the quality, intensity and complexity of stimuli. (Occupying center of interest from 1865 to about 1888.)
2. The effect of direction of attention on reaction times. (1888-1905.)
3. The introspective analysis of the reaction. (1905-19—.)

The emphasis or ascendency of the several phases of the main problem has shifted from time to time and at present experimentation covers all the aspects of the problem. It seems wholly unnecessary in a study of this kind, to give a detailed bibliography of the general subject of reaction time. Such a bibliography has been compiled by recent writers on the subject.[5] Reference will be made to such articles as have a particular bearing or interest in connection with the subject at hand.

Donders and deJaager were among the first to show that the difficulties in the study of reaction time were due to the complexity of the problem, the kind of stimulus, the mode of reaction, the degree of attention, and many other factors would alter the results. They also decided that the reaction time method was impracticable as a means of determining the rate of nervous impulse. At about this time the physiologists ceased to be interested in the problem, so that experimental work on reaction time came to be carried on largely by the psychologists. Donders and de Jaager, followed by others, adopted the use of reaction times as a method of determining or measuring more complex processes, notably discrimination and choice.

The earliest recorded experiments using the two hands is that of Donders.[6] If one stimulus appeared the subject was to react with the left hand, if the other appeared the subject was to react with the right hand. He assumed that this involved discrimination and choice, and that by subtracting from these results the results of simple reaction times, the time for discrimination and choice would be established. The study does not bear directly on the problem of finger or hand differences, however. This problem received little attention during the earlier years of the experimental work on reaction times.

---

[4] Henmon, V. A. C., "The Psychological Researches of J. McKeen Cattell," Archiv. of Psych. No. 30, April, 1914.

[5] Wells, G. R., Psych. Rev. Monog. v. 15, No. 5, 1913.
Moore, T. V., Psych. Rev. Monog. v. 6, No. 1, 1904.
Salow, Paul, Psychologische Studien, Bd. 7, 1912.

[6] Donders, Arch. f. Anat. u. Physiol. 1868.

Wundt[7] in the first study that appears in the Philosophische Studien emphasized the importance of studies in time measurements in psychological research and from the beginning, it was a subject of research in the Leipzig laboratory. Nor has the importance of this field of investigation ceased to be pointed out. Watson[8] emphasizes the importance of experimentation on conditioned reflexes and reaction times. His attitude differs from that of Wundt, Titchener and others. Watson's interest leans strongly towards the physiological. Cattell expresses a similar attitude on the importance of this field, an attitude that has dominated his work, "these sciences (i.e. physiology and psychology) cannot rank coördinate with the physical sciences until they consist of exact measurements."

Although the problem of reaction times has been considered important and has been the subject of investigation for the past quarter century, very little has been done to investigate the relation of symmetrical movements, executed either simultaneously or in insolation. The problem of attention occupies most of the literature on reaction time of this period.[9]

Among the earlier studies was that of Cattell[10] who produced a large number of reactions from each of two subjects, D and C. In the first series an electrical stimulus was applied to the several parts of the arm, in the second series the stimulus was touch.

---

[7] Wundt, W., "Über Psychologische Methoden," Philosophische Studien, Bd. I.

[8] Watson, J. B., "The Place of the Conditioned Reflex in Psychology," Psych. Rev. v. 23, 1916.

[9] Ach, N., "Über Willenstätigkeit und Denken," Göttigen, 1905.

Woodrow, H., "The Measurement of Attention," Psych. Rev. Mon. v. 17, No. 5, 1914.

[10] Cattell, J. McKeen, "On Reaction Time," Memoirs Nat. Acad. Science, v. 7, 1893.

The chief purpose of these experiments was to measure the speed of nervous impulse, but the experimenters decided that the results did not warrant any conclusions in regard to specific rate of nervous impulse, in fact that the method of reaction time was not a valid one for measuring the speed of the nervous impulse at all.

In a later work, Cattell and Dolley[11] further demonstrated the fact that the reaction time method is impracticable as a method of determining the rate of the nervous impulse. In investigating the problem as to whether the reaction time is shorter when the stimulus is given to the reacting hand than when it is applied to the other hand, they obtained positive results and also concluded that there is no difference in the reaction times of the two hands.

At about the same time (1893), Sanford[12] experimented on the effect of stimulating the reacting hand or the opposite hand, on the reaction time, the obtained results in accord with those of Cattell and Dolley. For electrical stimulation he found that the reaction time was 10 sigmas shorter when the stimulus was given to the reacting hand than when it was given to the opposite hand. He made no direct study of the difference in reaction times between the two hands, but a comparison made on the basis of his tables shows the relative reaction times for the right and left hand, for he, (Sanford) performed his tests with both the right and left hands as the reacting hands. When the stimulus was applied to the reacting hand for:

Subject S, R.H. is faster than L.H.
" B, R.H. " slower " L.H.
" R, R.H. " slower " L.H.

When the stimulus is applied to the opposite hand from the one that reacts, the following is true:

Subject S, R. H. is slower than L. H.
" B, R. H. " faster " L. H.
" R, R. H. " slower " L. H.

---

[11] Cattell, J. McK., and Dolley, S. C., "On Reaction Times and the Velocity of the Nervous Impulse," Psych. Rev., v. I, No. 2, 1894.

[12] Sanford, E. C., "On Reaction-times when the stimulus is applied to the Reacting Hand," Am. Jour. Psych., v. V, 1893.

His summary table of results is as follows:

TABLE A—Reaction with Right Hand
(after Sanford)

Subject	Stim. in R. H.			Stim. in L. H.		
	number reactions	average time	av. var.	number reactions	average time	av. var.
S	10	128.4	8.	10	142.7	9.
B	22	145.8	10.1	21	143.4	6.8
R	11	153.3	6.6	13	150.	6.1

TABLE B—Reaction with Left Hand
(after Sanford)

Subject						
	number reactions	average time	av. var.	number reactions	average time	av. var.
S	10	141.9	10.9	10	140.2	5.8
B	22	148.2	10.9	24	131.3	8.1
R	19	138.1	10.9	23	136.6	8.6

All times are given in sigmas.

Sanford accordingly concludes that it makes little difference which hand is stimulated and that the one hand is as fast as the other in reacting. It should be remembered however that the experiments which Sanford performed were with electrical cutaneous stimulation only, and it is not safe therefore to infer that these results are likewise true for other stimulation. Moreover the number of reactions is not large enough to be conclusive. An average of ten or twenty trials leaves a great liability of error. These investigations on the effect of stimulating the reacting or the opposite hand were first begun by Exner[13] and it is an attempt to repeat Exner's experiment that Sanford has made here.

Féré,[14] found that the reaction times for the two hands are different, the left being the longer. He also found that the reaction time for the two hands when acting simultaneously is longer than when each reacts separately. The right hand, when reacting alone, has a reaction time of 120 sigmas, when it reacts with the left hand 140 sigmas. The left hand when reacting alone,

[13] Exner, Sigmund, "Experimentelle Untersuchung der einfachsten psychischen Processe," Pflüger's Archives, VII, 1873.

[14] Féré, Ch., "L'energie et la vitesse des mouvements voluntaires," Revue Philosophique, vol. 28, 1889.

has a reaction time of 160 sigmas and when reacting with the right hand 180 sigmas.

He further investigated the effect of warming the hand on the reaction time. He found that after warming the slower hand (the left hand) that the reaction times equalled those of the right hand in speed and in some instances passed them. In giving a record of the effect on the several fingers of the left hand, he gives the reaction times for each finger before the warming process as: thumb, 346 sigmas; index finger, 269 sigmas; middle finger, 266 sigmas; ring finger, 255 sigmas; little finger, 283 sigmas. This very data shows that he found finger differences. Unfortunately he does not publish values for the fingers of the right hand, so that a complete set of values is not accessible from his experiments. He is one of the earliest writers to note the relation of the reaction time problem to the development of technique by the pianist.

Ach[15] in a set of experiments in which the subjects were to react as soon as they recognized the color of the card presented, had the subjects react with the right hand and the left hand on alternating days. His results showed that for subject L, the reaction time for the right hand was 1 sigma longer than for the left hand and for the subject H the reaction time for the right hand was 44 sigmas shorter than for the left hand. In another set of experiments, reported earlier in the same volume, he compares the reaction times for the thumbs and index fingers of the two hands and reports that for subject H the left hand is the faster by 54 sigmas, for subject J the left hand is faster by 56 sigmas and for the subject L it is faster by 2 sigmas.

Probably the most extensive work on the difference between the two hands is that done by Kiesow[16] in the laboratory at Turin. So far as the records show he was the first to investigate the reaction of the separate fingers in a comparative manner. For this set of experiments he was the observer. His results were as follows:

---

[15] Ach, N., Op. cit., reference footnote 9.

[16] Kiesow, F., "Beobachtungen über die Reaktionzeiten momentaner Schalleindrücke," Arch. f. d. Gesam. Psych., Bd. XVI, 1910, pp. 352-375.

Finger	Right Hand		Left Hand	
	average	mean var.	average	mean var.
1	171.7	19.5	172.4	16.8
2	175.2	17.	178.	13.5
3	166.8	18.5	173.8	13.
4	170.5	16.3	176.2	16.5
5	175.3	19.5	168.7	18.2

Kiesow concludes that the fingers of the right hand give shorter reaction times than those of the left, with the exception of the little finger of the left. He goes on to explain however, that the physiological condition of the little finger of his left hand may not be the same as in other people, due to his long continued practice on the violin. "Durch vieles Klavierspielen habe ich freilich alle meine Finger gleichmäsig geübt, aber ich habe in meiner Jugend auszerdem sehr viel Violine gespielt und hierbei die Bewegungen des linken kleinen Fingers in besonderem Mase üben müssen." In this work he used sound stimuli. He further tried out the difference between the two hands in a set of experiments with four right-handed subjects and three left-handed. The index finger of each hand was used to make the response, and the reaction type[17] he records as motor, i.e, the attention[18] directed towards the muscular reaction. The results were as follows:

Reaction-time of right and left hands to auditory stimuli
(after Kiesow)

Subject	Right Hand		Left Hand		Difference
	Average	M. V.	Average	M. V.	
J.C. (r.h.)	105.3	5.3	109.3	5.5	4.
C.S. (r.h.)	105.2	6.2	109.8	6.4	4.6
L.B. (r.h.)	105.6	5.3	107.6	6.	2.
M.P. (r.h.)	108.	6.9	111.1	6.5	3.1
R. (l.h.)	116.5	8.	107.5	7.	—9.
M.M. (l.h.)	113.4	8.5	110.4	8.6	—3.
P.E. (l.h.)	114.8	8.5	111.6	7.8	—3.2

---

[17] Salow, Paul. "Untersuchungen zur uni- und bilateral Reaction," Psychologische Studien, Bd. VII, p. 65, ff.

[18] Breitweiser, J. V., "Attention and movement in Reaction Time," Arch. of Psych., no. 18, Aug. 1911.

His results showed that in right-handed people, the right hand is faster and in left-handed people the left hand is faster. In indicating differences in tables, positive values mean right hand faster, minus values mean that the left hand is faster, by the amounts indicated.

Poffenberger[19] found a slight, but what he considered significant, difference in the reaction times of the right and left hands. He gives results from 100 reactions each from two right-handed subjects and 200 reactions from one left-handed subject. He obtained greater differences when only one eye was stimulated, particularly if the eye opposite to the reacting hand was stimulated, but this involves the question of indirect pathways as compared with the direct. This difference he regards as the time lost in conduction in the nerve centers, or the synapse time. His results were as follows:

Reaction-times of Right and Left Hands (Both Eyes Stimulated) (after Poffenberger)

Subject	Right Hand		Left Hand		Difference
	Average	M.V.	Average	M.V.	
T (r.h.)	184.4	9.9	184.7	5.2	.3
P (l.h.)	160.4	4.4	162.5	5.2	2.1
A (r.h.)	178.9	6.5	177.2	5.	—1.7

His differences were even smaller than those found by Kiesow. Subject P, who was left-handed, was faster with the right hand by 2.1 sigmas, and subject A, who was right-handed was faster in the left hand by 1.7 sigmas. In T there is no real difference. In fact none of the differences are large enough to be of great significance.

Sinn[20] concluded that the left hand has greater capacity for executing automatic and reflex movements and the right greater for consciously motivated movements. While Sinn does not offer an explanation or further substantiate his point it is in agreement with the theory advanced by Meyer[21], namely that during infancy the child is normally left-handed. If this be true, one might expect that the simple reflex movements be faster with the left hand, it being innately the preferred hand. However, the fact which Meyer also points out, that the number of fibres in the sartorious muscle, although greater in the left side in infancy, becomes considerably greater in the right in the adult, rather tends to substantiate a greater possibilty for the right hand in the adult. All experimental work has been done on the adult.

---

[19] Poffenberger, A. T., "Reaction-time to Retinal Stimulation," Arch. of Psych., no. 23, 1912.

[20] Sinn, Monatschr. f. Psych. u. Neur., Bd. 26, 1909, S. 234, ff., 321, ff., 430, ff.

Salow[22] agrees with Sinn in that the left hand has superiority over the right for reflex movements, and he further adds that the left hand reacts at almost its maximum speed from the first, but that the right hand may improve with practise although it will never quite reach the speed of the left. In motor reactions and choice reactions the difference is not significant.

Moore[23] has pointed out the importance of the movement itself. Many investigators in reporting their results have not been careful to state what movements were involved in the reaction. Cattell in his work with Dolley on reaction time as a means of measuring speed of nervous impulse, found that the reaction time is longer when the movement is made from the shoulder, less for the forearm and nearly the same for the wrist and fingers. When a subject is told to press down a key, various sets of muscles may be employed, unless some precautions are used to have the subjects use the same movement. The position of the arm will make some difference and cannot always be entirely controlled. Barnes[24] in his study of reaction with isolation of a muscle group controls more or less satisfactorily the position of the arm and the group of muscles. However this is an artificial condition, one not found in ordinary reactions with which we are concerned, so that there is something to be said on both sides of the question of artificial control of this kind.

---

[21] Meyer, M. F., "Left-handedness in Infancy," Psych. Bul. vol. 10, 1913, p. 209.

[22] Op. cit. reference footnote 17, p. 80, ff.

[23] Op. cit., reference footnote 5.

[24] Barnes, J. C., "Voluntary Isolation of Control in a Natural Muscle Group" Psych. Rev. Monog., v. 22, no. 1, 1916.

The study made by Barnes is a most interesting one in its relation to piano technique. His experiments deal with the control over special muscles in a group which ordinarily contract together. Most of his discussion and conclusions are based on the introspective data recorded by the observers, but the methods used and the ultimate results are related to the present topic. He found among twenty-two men and ten women subjects no sex differences. He selected the ring finger because it usually has the least independent action of any of the fingers. The experimental performance consisted of moving the ring finger. first in vertical and second in horizontal direction, without movement of the middle and little fingers. The movements of all three fingers were recorded on a drum. At first there were slight movements of the ring finger and many movements of the two adjoining fingers. As the practise increased the movements of the middle and little fingers became less and those of the ring finger wider in amplitude, until perfection in the appointed task was attained. After perfection of the movement by the right hand, the left was given the same experiment. The results showed that when the left hand was placed in the thimbles, the ring finger moved almost as easily and with as great aptitude as though it were continuing the practise of the finger of the right hand, which had been going on for two months. After five trials the movement on the left hand was practically perfect.

The lateral movements of the ring finger are not so important, but a brief summary of results of this group of experiments shows that although the ligaments binding it to the little finger and the middle finger are so arranged that any extensive movement of the ring finger in the lateral direction would always pull one or the other of the two adjacent fingers, nevertheless the movement was learned much more rapidly than movement in the vertical plane. This speed in learning the control of the movement probably was accelerated by the previous experiment, although as Barnes says, it is hardly safe to assert this definitely. For all of the observers the same order was maintained, so that no data is given which offers any information on the relative time of learning for the right and left hands, when they are the initial performers. In all of his cases the right hand had mastered the movement before the left hand began it.

Practically nothing in the way of research has come from the pianists themselves. It is evident however that some of the best teachers and performers have recognized the problem and the difficulties that it offers to the study of piano technique. Patterson[25] writes thus: "The great secret of beautiful playing on the piano lies in the independent and well regulated touch of each finger on the hand. To acquire this is no easy thing. It may take months and even years of patient practise." The writer goes on to explain that the five fingers of the hand are unequal in size, shape and capability. Their positions in regard to the palm of the hand, as in connection with each other, give to every member of the group an individuality which we emphasize when we speak of the thumb, the index, the middle, the ring and the little fingers. "The pianist, at the commencement of his study, knows how much easier it is to strike a note firmly and clearly with the first and second fingers (usual musical notation of fingers) than with those known as the third and fourth. The reason for this is explained upon examining the tendons of the hand. Both the third and fourth fingers are more fettered by digital ligaments than are the remaining two and the thumb. The hand is constructed rather to grasp or to hold than to strike, in the sense in which we attack ,or touch the keys. Preliminary aids to strengthening the arm muscles, and therefore the hand and wrist action, have been found helpful by many executants."

W. Mason in the same volume[26] in a chapter on the "Two-Finger Exercise" suggests something of the problem of equalizing fingers. Liszt, the great master, says of this two-finger exercise, "There is one little exercise which has come down from Hummel that I never give up. It does one more good than anything else." Lizst is reported to have played it without rhythmical form, just as a gymnastic exercise to increase the uniformity of the fingers. Mason suggests other forms of so-called gymnastics for the same purpose, i.e., increasing the uniformity of the fingers. This is the fundamental problem which confronts the pianist in developing technique.

---

[25] Patterson, A., "Theory of Music and Piano Technique," Encyclopedia, edited by Elson, pp. 193-195.

[26] Mason, W., Op. cit., reference footnote 25, pp. 218, ff.

Such indefinite discussions as these are practically all that one finds in the musical literature. Such studies as that of Pond[27] even are not many. His article is one of the few attempts to study music or musical methods from an experimental standpoint. The study is of performance on the French horn, chiefly, and the author points out the value of introspective analysis by a trained observer of the methods of controlling the mouth, lips, breath, and to some degree the fingers. It is to be hoped that many more such studies may follow as aids in developing methods of technique for the various instruments.

APPARATUS

The apparatus used for this experiment was that which was designed by Dr. A. P. Weiss for experimental work on conditioned reflexes. It was so nearly like that needed for the present research that Dr. Weiss offered its use and made the necessary modifications. Fig. I shows the electrical connection of the apparatus. It consisted of three parts:

1. The stimulus mechanism (shown in unbroken lines)
2. The reaction mechanism (shown in broken lines)
3. The calibration circuit (shown in dotted lines)

1. *The stimulus mechanism*—The D. C. current entering at point *a*, is passed to the tripper magnet of *A*, which is a horizontal duplex pendulum, so weighted that when the current is passing through the magnet, end *x* of the bar is pulled downward. From tripper magnet A, the current passes to *b*, where it may take one of three branches. The branch *b, l, c* passes through the left signal relay, then through left signal marker and the resistance light 1, to the other terminal of the D. C. socket. In operation this branch gives the left signal and also records it on the paper record strip.

---

[27] Pond, S. E., "A Contribution to the Study of Instrumental Music," Jour. of Applied Psych., v. II, 1918.

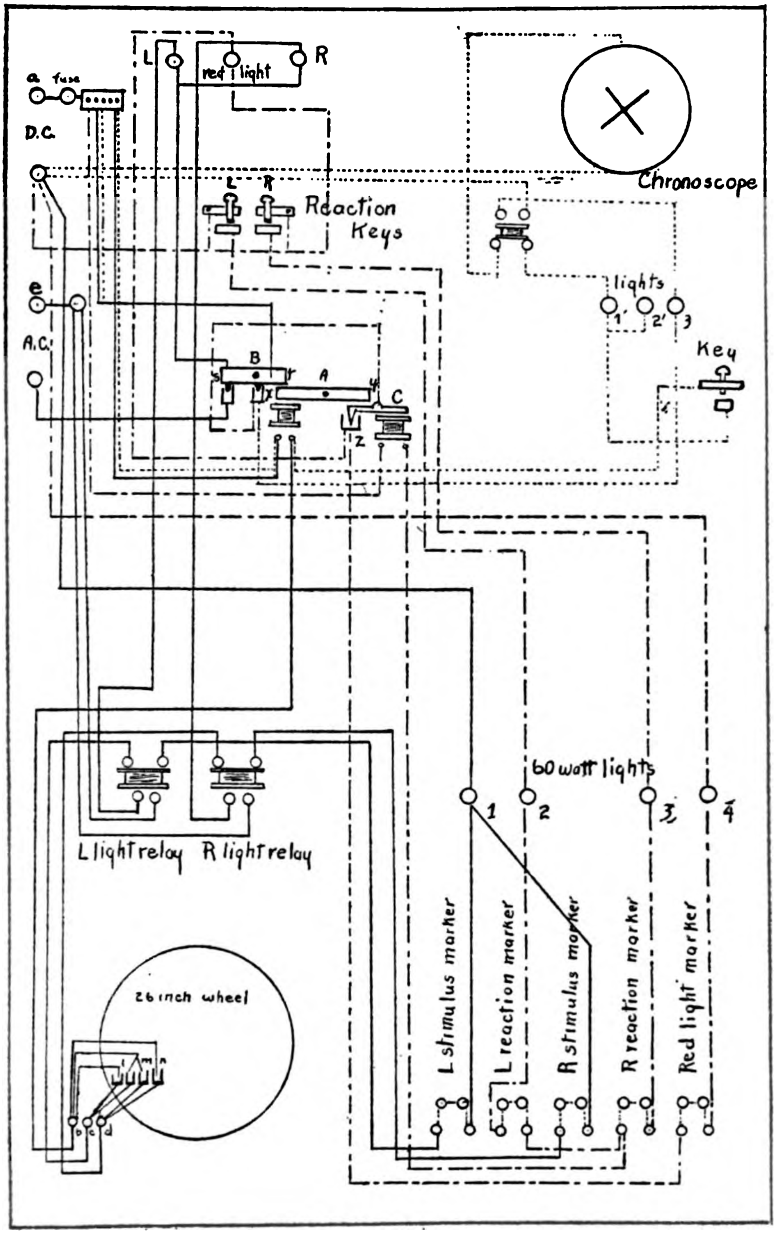


FIG. I.—Diagram of electrical connections for whole of apparatus.

The branch *b, n, d* passes through the right signal relay, then through the right signal marker to the resistance light *1* as above. in operation this branch gives the right signal and also records it on the paper record strip. The branch *b, m, c-d* is merely a combination of both the above and gives a double stimulus and its record.

The large wheel represented in the diagram consists of a flat wooden disc (26 inches in diameter). On this wheel are three rows of screws at varying intervals. Above the edge of the disc are arranged three mercury contacts, *l, m, n.* As the wheel revolves, one or another of these cups is pushed up far enough by the screws to make contact with the hook extending over it. The wheel is revolved by a motor placed under the table. This wheel serves to vary the succession of stimuli presented to the subject and also the time interval between the stimuli. It makes it possible to retain the same order for *each* subject throughout the experiment.

The A. C. current enters at *e,* part going to each light relay. From the right (R) light relay, the current passes to the stimulus light R in the dark room (Fig. V) and from L light relay the current passes to stimulus light L in the dark room. From stimulus light L, the current passes to *B,* the Timer. So long as the *x* end of the tripper *A* is up, the current will pass through the mercury cup at *s* in *B,* and thence outwards. However as soon as the magnet pulls the *x* end of *A* down, then the connection of the A. C. current passing to the stimulus light L is broken at *s.* The object of the break at *s* is to give a stimulus of constant duration independently of the rate of rotation of the wheel. That part of the A. C. current which passes through R light relay to stimulus light R, in the dark room, takes the same path to *B,* and then outwards as that taken by the circuit from stimulus light L.

2. *The Reaction Mechanism*—The reaction mechanism consists of that part of the apparatus which records the *responses* given and the presence or absence of the red light which marked the time limit. The current (D. C.) entering at *a,* passes by one branch to the magnet of *C,* the red light tripper, thence to the

left light or right light reaction marker, depending on which reaction key, L or R, is depressed by the subject. From the marker, the pathway is through the 60 watt lights *2* and *3*, thence to the keys indicated and from there outward.

The other branch (D. C) from *a*, passes to *B*, the timer, then through the mercury cup at *t*, which is in series with mercury contact *z*. If the keys are depressed by the subject, the magnet *C* jerks the hook of *C* out of contact with the mercury cup. If the subject does not depress the keys, and thus direct the current through the markers L and R, the current passes on through the red light and to the red light marker, and then outward through light *4*.

By this method, if the circuit is completed by the depression of the key, the appropriate reactions are recorded. If the circuit is not completed in time, the red light flashes and a "slow" reaction is recorded.

3. *Calibration Circuit*—The calibration circuit, represented by dotted lines, is that part used to regulate the time interval between the stimulus light and the red light. In this experiment the time interval used is four hundred sigmas.

For this circuit the current (D.C.) leaves *a* by the same branch as the beginning of the stimulus mechanism and passes to the magnet of tripper *A*. From here it passes to a key, *K*, indicated in the diagram. When this key is depressed the circuit is closed and the tripper arm of *A* releases the arm of *B* and at the same time starts the chronoscope. As soon as the hook at the *t* end of *B* comes into contact with the mercury cup, the chronoscope relay and light *3′* breaks the time recording circuit.

The chronoscope thus measures the time between the passing of the current through the magnet of *A*, which on the stimulus mechanism controls the stimulus lights, and the time of contact at the *t* end of *B*, which in the reaction mechanism controls the red light. That is to say, the chronoscope measures the length of time it requres the hook at *B* to drop from its upper position to contact with the cup.

Lights 1′, 2′, and 3′ are inserted to increase the resistance. By means of a screw device the mercury cup can be adjusted until the time of fall recorded by the chronoscope is exactly four tenths of a second (400 sigmas). Before each series of experiments the apparatus was calibrated to the desired time interval.

The Record—By means of a roller moved by the motor under the table, a paper feeding device is used which unrolls a record strip before the markers. These markers consist of glass tubes drawn out to a fine point, and then filled with ink. At the contraction of the magnet the attached point is projected against the paper, making a dot. L *stimulus* marker carries blue ink, L *reaction* marker carries red ink, R *stimulus* marker carries blue ink, R *reaction* marker red ink, and Red light marker blue ink. This gives a clear and simple method of recording the exact results of the reactions as well as the stimuli.

The following illustration shows all the possibilities, i.e., all combinations of stimuli and reactions:

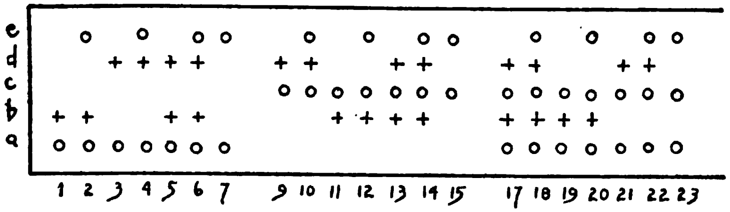


FIG. II.—Specimen record showing all possible reactions.

A dot in column *a* (bottom row) indicates an L light *stimulus*, a dot in column *b* indicates a *reaction* on the L key. A dot in column *c* indicates an R light *stimulus*, one in column *d* a reaction on the R key. A dot in column *e* indicates the flashing of the red light, which means that the reaction has been made more than four tenths of a second after the stimulus.

When a given response in a record was marked 1, it meant that the left light in the dark room had flashed on, and that the subject had pressed down the left hand key in less than four tenths of a second. If however, it was marked 2, it meant that the left light had flashed and that the subject had pressed down the left hand key, but that the time interval had been more than four tenths of a second. Number 3 means that the left light flashed, but that the subject depressed the right key instead of the left. This method of notation is used throughout in marking records.

By this record system, electrically controlled markers, that are a part of the stimulus and the reaction circuits, make it possible to know in each reaction, what has been the stimulus, what the response and whether the reaction time was greater or less than 400 sigma.

A key to the record is furnished by the following table, in which the numbers refer to the numbers at the bottom of the sample record of Fig. II. For the sake of simplicity the reactions are grouped by omitting numbers 8 and 16.

1. Left light stimulus, correct response.
2. " " " , " " , but slow.
3. " " " , right key response, not left.
4. " " " , right key response, and slow.
5. " " " , both keys depressed.
6. " " " , both keys depressed, slow.
7. " " " , no reaction.

9. Right light stimulus, correct response.
10. " " " , correct response, but slow.
11. " " " , left key response, not right.
12. " " " , left key response, slow.
13. " " " , both keys depressed.
14. " " " , both keys depressed, slow.
15. " " " , no reaction.

17. Both lights stimulus, correct response.
18. " " " , correct response, but slow.
19. " " " , left key only.
20. " " " , left key only, slow.
21. " " " , right key only.
22. " " " , left key only, slow.
23. " " " , no reaction.

By means of the wheel device already explained, the order of the stimuli is varied, but varied in a constant manner so that the subject can not tell what stimulus is to come nor the exact time when it will appear. This eliminates preliminary preparation. A portion of a sample record is shown in Fig. III.

Method of Scoring.—If the correct response is given within the time limit, 400 sigmas, it is counted as perfect. If the correct response is given, but slower than the time limit, it is counted as accurate. Comparisons are made on the basis of

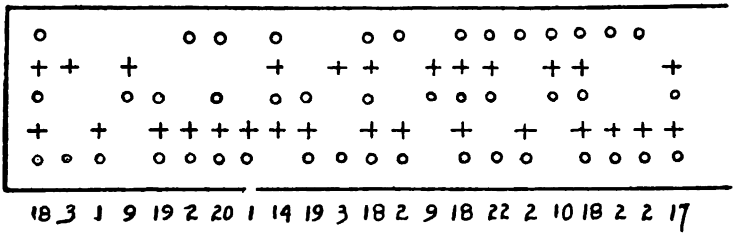


FIG. III.—Portion of sample record.

score, and in all cases scores are converted into percentages. Thus 46 percent perfect reactions means that out of 100 stimuli presented, the subject reacted with 46 perfect scores. This method of scoring is based upon the principle of standard, or "how many fingers reach or pass a set limit."

The first preliminary set was given with the time limit of 300 sigmas, which time was found so fast that very few perfect responses were made by the subjects. The second preliminary set was then given with the time limit of 400 sigmas and showed that the average subjects ranged about 50 percent perfect responses in the trial series. In the final series, one subject who took the experiment with the limit of 400 sigmas, showed a very high percentage of perfect reactions, but when tried out on 300 sigmas, gave too few perfect reactions to give satisfactory results for comparative purposes. One subject, (Go) was given the entire series with the time limit of 300 sigmas. All of these times may seem long, but the reaction is such that it becomes a choice reaction and not a measurement of simple reaction time. This explains the longer time required.

SUBJECTS

Fifteen right-handed subjects were used, eleven women and four men. Eight subjects took all nine series, i.e., all forty-five combinations, and five took two series, i.e., ten combinations. Two others took only one series each and these results were used only in the preliminary study. One subject (Ga) took the entire forty-five combinations twice.

The personnel of the group of eight, that group, the results of whom have furnished the basis of this report, was as follows:

We. was a professor of psychology at the Ohio State University. He had no piano training, but was an expert mechanician, skilled in the use of hands and fingers for fine adjustments and reactions.

Mu. was a graduate student, doing research work in the department of psychology during the summer session of 1918. She was thoroughly familiar with the apparatus and its use.

Wo. was a student at the University, doing undergraduate work. She was a teacher in the public schools, giving some time to public school music. She played the piano a little, although her training had been on the reed organ more than on the piano.

Mi. was a graduate student, assistant in the department of psychology. She was thoroughly familiar with the apparatus and its use.

Ak. was a student at the University. She was a pianist of considerable ability and also a teacher of piano. She was one of the most practised subjects used.

Go. was a student at the University. He had had no specific training, but his interest in experimental work prompted cooperation as subject.

Ki. was a student at the University, doing undergraduate work. She was a teacher of piano and was interested in the problem for its bearing upon piano technique. She is a pianist of several years training, also a performer on the pipe organ.

Ga. was a graduate student, the writer of the present article. She had studied piano, and violin, had taught piano, and it was her interest in the development of methods of technique that first stimulated interest in this study.

The other subjects used included two instructors in the psychology department during the summer session 1918, Cu. and Ge. The rest were students, with some piano training, two (Si. and Be.) being teachers of piano work as well as performers.

The writer wishes here to express her appreciation of the services rendered by those who gave so cheerfully of their time and effort, and who by their hearty coöperation have made this study possible.

METHOD.

So much confusion has arisen in designating fingers in work of this kind, (particularly where both hands were used) that the following method has been adopted for the present report.

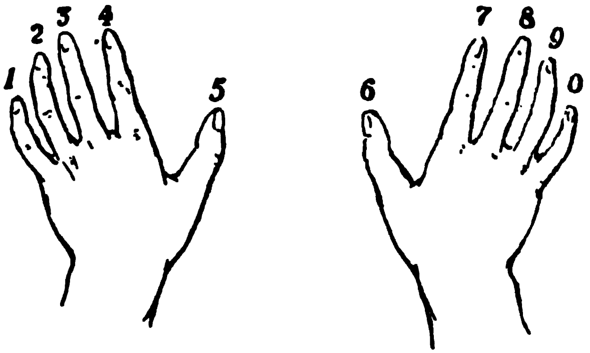


FIG. IV.—Finger notation used throughout this report.

When reference is made to combination 24, for example, the ring finger and the fore finger on the left hand are meant. When reference is made to combination 50, the thumb of the left hand and the little finger of the right hand are meant, and so on for the various combinations. Forty-five combinations are possible, and experimentation was made on the entire number. These forty-five combinations are grouped in sets of five each, making nine sets. Each group of five made up a series. Four subjects took the series in order I to IX and the rest in the order IX to I, but no two subjects had the same order of combinations within a series. The numbers used refer to the diagram and this method of designating the fingers will be used throughout this work.

Each subject was taken into the dark room, in which the keyboard end of the apparatus was located. A single 40 watt tungsten lamp was used to light the dark room, so that the illumination was about half that ordinarily used in a room. The light was placed behind the subject. The arrangement of stimulus lights and reaction keys is shown in the diagram of Fig. V.

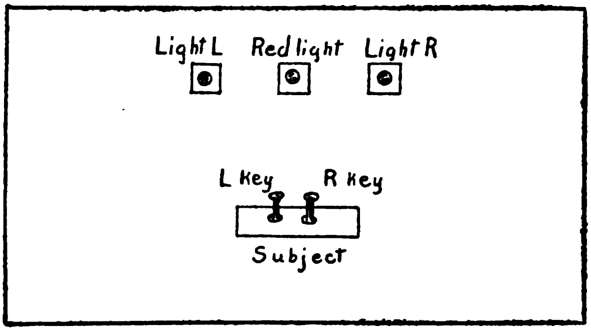


FIG. V.—Diagram of arrangement of lights and keys in the dark room.

The two 10 watt Mazda lights used as stimuli were placed in long wooden boxes with equal sized holes in the ends, so that the subject saw not the glare of the filaments of the bulb, but rather the glow of the yellow light that filled the box. The placement of the red light was in a similar box. The keys with which the subject reacted were of the depression key type, ordinarily used, but so arranged, by the tension of the spring that it required 70 grams weight to depress each key. The amount of weight required to depress the keys of pianos of different makes varies somewhat, but the variation among the keys of any given piano of good quality is small. A Knabe grand piano which was tested required a weight of 53 grams to depress the keys. Only a few keys varied from this amount and none by more than six grams. Likewise a Chickering grand piano which was tested required a weight of 37 grams to depress the keys, and showed no variation greater than 7 grams. The keys used in this experiment requiring 70 grams weight have enough resistance to equal that of any of the pianos of so-called stiffer action and yet is not equal to the amount usually given by all the subjects in reacting to keys of this sort. In other words, the resistance was not enough to cause errors due to failure to depress the keys far enough to make electrical contact. Whenever the finger reacted to the key, it was recorded.

The following instructions were given to each subject at the beginning of the first series:

> You are to sit in here thus: (experimenter takes place in the chair, places hand on the keyboard and demonstrates). Now in the box on the left there is a yellow light, and in the box on the right there is a yellow light. When the light comes on in the left-hand box, press down the left-hand key as quickly as possible; when the light comes on in the right-hand box, press down the right-hand key as quickly as possible. When the two lights come on at the same time, press down both keys as quickly as possible and as nearly at the same time as you can. Try to avoid tripping, i.e., putting one key down before the other. Now in the middle box there is a red light. This will come on when the responses which you have made to the yellow light have been too slow. You are to try to make the responses as accurate as possible and at the same time to keep the red light from flashing in as many cases as you can.
>
> In each set you are to use a different pair of fingers; I shall indicate which finger combinations to use, just before each series is begun.

After the first series it was unnecessary to repeat the instructions. Merely a reminder of the stimuli, the responses required and a naming of the fingers to be used were given. A rest of 60 seconds was allowed between combinations, and the first six or eight reactions were not counted. Each series, i.e., each set of five combinations, was given at a separate time, usually on succeeding days. A single series required forty minutes for execution.

RESULTS

Difference between fingers on the basis of speed.—A comparison of the relative speed of the fingers is shown in Table I, which gives the rank of each finger for each individual.

TABLE I.
Rank of Individual Fingers on Basis of Speed.

Subjects	We	Wo	Mu	Ga	Mi	Ak	Go	Av. Rank	A.D.
Finger 1	10	7	6	8	8	10	10	8.4	1.3
2	5	10	10	5.5	9	3	4	6.6	2.6
3	8	9	4	10	4	7	7	7.0	1.7
4	7	8	9	1	7	4	5.5	5.9	2.0
5	9	6	8	4	10	8	2	6.7	2.3
6	2	3	5	2	1	5	5.5	3.3	1.5
7	4	4	3	9	3	2	1	3.7	1.6
8	6	5	2	7	6	1	8	5.0	2.0
9	3	2	7	5.5	5	6	3	4.5	1.5
0	1	1	1	3	2	9	9	3.5	2.9

Correlations on the basis of the relative rank between the several fingers when used by the different subjects does not give high correlation except in a few cases. This only means that individuals differ among themselves as to the relative skill of the fingers. It is however noticeable that certain fingers are usually the best fingers, the variation being greatest among the rest of the fingers. The number of right hand fingers placed in the upper 50 percent of the ranking is as follows: We, 4; Wo, 5; Mu, 4; Ga, 3; Mi, 4; Ak, 3; Go, 3. The same numbers are accordingly found for the left hand fingers in the lower 50 percent of the ranking. The subjects that show the greatest speed also show greater distribution of the fingers of the two hands, e.g., Ga., Ak., and Go.; and the subject that was the slowest throughout, Wo., shows the greatest grouping, all five fingers of her right hand being in the upper 50 percent of ranking.

Comparison of Right Hand with Left Hand Fingers.—In agreement with the findings of Kiesow, the present results show differences not only between hands, but also between the fingers of the two hands. These differences are best shown by Table II. The results are given in terms of percent, which value is derived by taking the sum of all the perfect reactions for a given finger in combination with each other finger and dividing the result by 450. The differences between right and left hands as shown here are based on 2,250 reactions for each hand.

TABLE II.

Differences between Fingers on Basis of Speed.

Subj.	Left Hand						Right Hand						Diff.
	1	2	3	4	5	Av.	6	7	8	9	0	Av.	
We	51	58	55	56	53	54	66	62	57	62	66	62	8
Wo	18	14	16	18	19	17	40	36	29	44	46	40	22
Mu	53	46	56	47	49	50	56	57	61	56	65	59	9
Ga	40	42	37	50	42	42	48	39	40	42	44	42	0
Mi	53	51	56	53	50	52	64	57	53	54	60	58	6
Ak	70	81	77	80	77	77	79	84	86	78	71	80	3
Go*	18	28	28	28	29	26	28	37	27	29	26	30	4

* The figures for Go. are the basis of a shorter time, 300 sigmas, but the differences between fingers and between hands is shown just as clearly.

The results show that the right hand excels with respect to speed in every case except one, subject Ga. Ga was a subject whose mother was natively left-handed, but who had acquired right hand habits through training. Ga's father was a member of a family that showed decided tendencies toward ambidexterity. One brother of Ga is left-handed. Moreover the training of the subject Ga has been such as to develop the reactions of the left hand. Piano practise, typewriting and other performances using the fingers of the left hand were habitual. In addition to this there is the factor which Kiesow mentions with regard to the development of the fingers of his own left hand, particularly the little finger, namely that the subject had in earlier years played the violin, which performance develops the reactions of the left hand fingers, especially the little finger.

It is again noticeable that the subject that showed the slowest reactions was the subject who showed the greatest differences between hands. (Wo., difference of 22). Likewise Mu, whose training next to Wo's was least complete showed a great difference between the performance of the two hands (10). It seems probable that as the practises which increase finger efficiency go on, the differences between the fingers and between the two hands grow less and less. *In other words, instead of resulting in a uniform increase with each finger, practice tends to equalize the reactions of the several fingers.*

With regard to accuracy, differences between fingers and between the two hands are again evident, but are much smaller than the differences in speed. The values are obtained by taking the sum of all the correct reactions, whether on time or slow, and dividing by 450 as in the preceding table of results. The differences in accuracy for the ten fingers are shown in Table III.

The differences here between the two hands are too small to have much significance. The fact that the percentages are so large throughout indicates that the method is not a sufficiently fine measure of accuracy. But it has the advantage of measuring this factor at the same time as the speed, i.e., in the same performance. The right hand is more accurate than the left except

TABLE III.

Differences between fingers on Basis of Accuracy.

Subj.	Left Hand						Right Hand						Diff.
	1	2	3	4	5	Av.	6	7	8	9	0	Av.	
We	81	85	86	80	87	84	90	86	86	88	86	87	3
Wo	89	89	89	88	88	88	93	90	89	90	88	90	2
Mu	89	91	91	90	76	87	86	87	92	87	89	88	1
Ga	92	93	93	94	95	93	93	95	94	93	93	93	0
Mi	80	84	86	83	79	82	86	86	85	83	84	85	3
Ak	91	94	94	92	93	92	95	95	96	95	89	94	2
Go	75	78	84	79	78	79	76	75	80	74	66	74	—5

in one case, subject Go. Subject Ga., as in the measurement of speed, shows no difference between the right and left hands.

The explanation offered there applies equally with respect to accuracy. A similar explanation might perhaps be offered with regard to Go, who showed the left hand to be more accurate than the right. His mother was left-handed and some other members of the family showed unusual skill with the left hand.

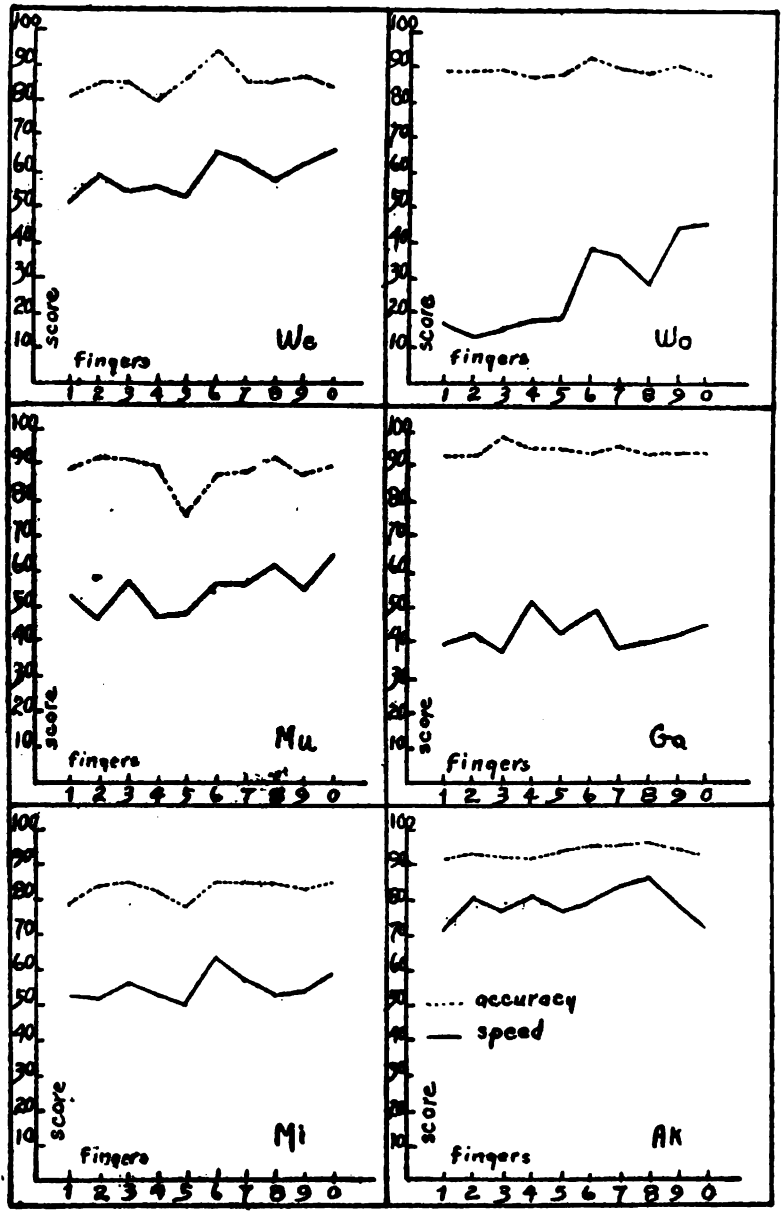
The differences between the fingers in speed and accuracy are shown in Graph I.

Fingers in Combination.—The finger with which a given finger is combined is a factor influencing the speed and to a lesser extent the accuracy of the response of a finger at any given time. Here as in other points there are individual differences. However there is indication that in the majority of cases, certain fingers are more easily combined with the thumb of the right hand for example than the other remaining fingers, etc.

TABLE IV.

Finger with which a given finger produces the highest speed.

Subject	1	2	3	4	5	6	7	8	9	0
We	0	7	9	0	9	4	2	6	1	1
Mu	0	7	7	0	8	8	2	6	5	1,4
Wo	6	7	9	6	0	4	9	5,6	7	4
Ga	0	7	9	0	9	1	2	6	5	1,4
Mi	3	6	9	1,7	8	1,4	3	9	3	5
Ak	8	7	6	9	8	4	3	2	1	2
Go	8	0	0	7	0	0	8	7	7	5



Graph I.—Differences between fingers in speed and accuracy.

TABLE V.

Finger with which a given finger produces the greatest accuracy.

Subject	1	2	3	4	5	6	7	8	9	0
We	3,7,0	6,7	1	1	7	2,4	2	2	1	1
Mu	6	7	5	6	7,8	8	6,9	7,8	8	6,7
Wo	1	4	8,9	2,6	4,7	2,4,0	0	3,5	0	5
Ga	7	5	1	7	1,2,4	3	5	3,4,6	2,6,7	1,6,8
Mi	7,8	1	7	7,0	8	0	3	4	0	1,2,5
Ak	7	7	6	0	6	5	2	9	1,9	1,4
Go	9	8	0	1,9	3,0	3,4	8	9	1,6	5,6

For three of the subjects, finger 1 obtains its greatest speed in combination with finger 0, for two others when with finger 8. Five of the seven subjects obtained the greatest speed with finger 2 when in combination with finger 7. It is not necessary to call special attention to the values for each finger as this can be derived from the table. The general conclusion to be drawn is the fact that the greatest speed is obtained when the given finger is *acting with a finger of the opposite hand.* For fingers 1, 4 and 6, six of the seven subjects show this to be true; for fingers 2, 3, 5, and 0, all seven subjects agree; and for fingers 7 and 9 five of the subjects find the greatest speed when the combination includes a finger of the opposite hand. Finger 8 offers a decided exception in that all but one subject produced the greatest speed with this finger when it was in combination with some other finger of the same hand (right hand). It is quite possible that the middle finger of the right hand has by continued use become more independent in its movements than the other fingers, and has developed skill in operation with the fingers of the same hand particularly. In grasping an object, manipulation of tools and other articles, the middle finger and the thumb are continually juxtaposed. Four of the subjects found that the greatest speed was obtained when finger 8 was in combination with finger 6, which represents the thumb of the right hand, one when in combination with the index and another when in combination with the ring finger.

There is little similarity among the subjects on the basis of accuracy (Table V). A much wider variety of combinations is recorded. The same general principle holds true, however, as in the case of speed, i.e., the greatest accuracy is obtained by most observers when the finger used with the given finger is on the opposite hand.

Comparison of Two-Finger with One-Finger Reactions.—Although responding to stimuli with two fingers requires more movement, and also the excitation of more pathways, nevertheless the results show that two-finger reactions are the faster as well as the more accurate. The method of derivation is: the sum of the 1's plus the 9's for the entire 45 combinations, divided by 4500 equals the percent of perfect scores for all single finger reactions involved; the sum of all 17's for the entire 45 combinations, divided by 2250 equals the percent of perfect scores for all double-finger reactions involved. The results are as follows:

TABLE VI.

Speed of Single-finger Reactions Compared with Two-finger Reactions.

Subjects	Single-finger	Double-finger
We	59	74
Wo	28	46
Mu	54	76
Ga	43	68
Mi	55	74
Ak	78	89
Go	28	41

TABLE VII.

Accuracy of Single-finger Reactions Compared with Two-finger Reactions.

Subjects	Single-finger	Double-finger
We	85	97
Wo	89	98
Mu	87	97
Ga	93	97
Mi	83	99
Ak	93	99+
Go	76	99+

With the score for one-finger reactions based on the results of 4500 single reactions, and the score for the two-finger reactions based on the results of 2250 reactions a considerable degree of reliability may be assumed. Cattell claimed that the variable error is practically eliminated in the average of 100 measurements for each. For every subject the two-finger reactions are much the faster, and are also more accurate. This might be explained in several ways: (1) increased stimulation, (2) nervous accumulation, (3) absence of the choice element.

Todd[28] found that whenever another stimulus was added to a given stimulus, there was a reduction in the reaction time. "That is to say, the added stimulus facilitated or reënforced the reaction-reflex." On this ground, it is reduced to the assumption that strong stimuli force the barrier across the synapses more quickly than do weak ones. However in the present work, whenever a double stimulus is given, a double reaction is also required. It would be difficult to determine how much of the speed gained in the two-finger reactions were due to increased stimulus, apart from other factors involved.

When the fingers are not reacting, both are in preparation for the coming stimulus, inasmuch as the subject has no means of knowing which stimulus will be presented next. If the stimulus that appears is a single one and for the right-hand finger, the other finger must be inhibited or kept from reacting. This is just as much a part of the process as the movement downwards of the approximate finger. Now, when a double stimulus appears, requiring the movement of both fingers, this element of choice and inhibition is lacking. The "set" prepared in both "releases the trigger" and the double movement occurs. This is one of the elements in the shortening of the time.

The instances in which only one finger responds to a double stimulus are very much rarer than those in which two fingers respond to a single stimulus. In those cases where there were two lights and only one finger reacted, it was more often the right finger than the left. This was true for five of the seven subjects. In those cases where only one stimulus light was given and two fingers reacted, the results are as follows:

With five of the seven subjects more errors of this kind occur when the right hand stimulus is given. In other words, the left hand makes more uncalled for reactions than does the right. This means poorer inhibition of the movements of the left hand, inasmuch as the "set" is opened for either reaction when the

---

[28] Todd, J. W., "Reaction to Multiple Stimuli," Arch. of Psych., v. 25, 1912.

TABLE VIII.
Double reactions to single stimuli

Subjects	Left stimulus both fingers reacting	Right stimulus both fingers reacting
We	18	13
Wo	9	10
Mu	9	14
Ga	6	6
Mi	16	17
Ak	5	7
Go	22	25

stimulus appears. This is another evidence of the less efficient control of the movements of the left hand as compared with the right. These figures represent the results of 2250 reactions converted into percentage on the basis of the whole.

Relative Efficiency of Combinations.—In another connection it was noted that the combination which is most efficient for different individuals may vary, but that is usually a combination involving one finger on each hand. Further examination of the relative score of the different combinations used shows that the individuals differ greatly as to the order of efficiency, both in speed and accuracy. The scores for each combination of fingers and for each subject when compared bear out this fact.

Correlation of the relative rank of the combinations for different individuals show for the most part low and in some cases negligible correlations. A few are high enough to be significant. This marked difference between individuals on the basis of combinations is probably due in large measure to a difference in training. When the fingers are considered apart from any particular combination, certain fingers seem always to excel, i.e., the same finger is found to be best for several individuals, and yet their most efficient combinations of fingers vary. No two people perform exactly the same acts or movements in ordinary life. The fundamental movements are similar and accordingly certain fingers excel in speed and accuracy. Moreover, certain combinations are higly developed in all subjects. For example, combinations 16, 10, 27, 40, 70, 39 and others occupy a relatively high rank with most subjects, while combinations 25, 15, 23, 29, 35, 45, 49 and others occupy a low rank with most of the subjects. These are the most highly developed and the lowest developed combinations. The lack of correlation between subjects on the basis of the whole forty-five combinations, is brought about by the wide variation in the rank of other less used combinations of fingers, which vary from subject to subject in degree of development.

The five subjects who took only ten finger-combinations, were given combinations of such a nature that they would include 4 right hand combinations, 4 left hand combinations and 2 two hand combinations. Although the figures are smaller here in the case of the first group of subjects, the results are worthy of mention. An average of the results of the several combinations and for the five subjects gives the following:

Combination	Speed	Accuracy
Left hand	60	90
Both hands	59	94
Right hand	44	92

The combination which were presented to these five subjects were simply selected as being those frequently used in piano, work, so that it is a mere element of chance so far as this experiment is concerned. When the relative rank of these individual finger combinations is noted in the records of those subjects who performed all forty-five sets, the above results are found to be in harmony. In other words, it so happened that the combinations chosen were right hand combinations which ranked low comparatively, and left hand combinations which ranked high. This merely shows then that there is a fair degree of reliability in the method even where only a few sets are required of each subject. It also demonstrates the importance of indicating the members of the body used in any given experiment. On the basis of one combination of fingers very different results are obtained from those using some other combinations and may lead to erroneous conclusions.

Some practise effect may be included in the score of the combinations that occur later in the series of forty-five combinations. This will effect to some small degree the relation of combinations, but in the total results this effect tends to be counterbalanced by the inverted order of presentation of the series to different subjects. Moreover, the order of combinations was so aranged that in almost every group of five, each finger was used once. There was a few modifications from this arrangement, but not variation enough to give one finger more practise than another. By this means, even though in the later series the fingers may have a slight increase in speed due to practise effects, it is uniform, so that in averaging results, this factor is alike for each finger.

Considering the twenty combinations which rank at the upper end of the scale for each individual, it is found that by far the majority of these combinations are combinations using one finger on each hand. For We and Ak, 16 of the 20 combinations are two-hand ones, for Wo, Mu, Ga and Mi, 13 of the 20 are two-hand combinations and for Go 12. Most of the remaining combinations that rank among the twenty fastest are right hand combinations, i.e., both fingers belong to the right hand. Only nine combinations out of the whole 140 (seven subjects with 20 combinations each) are left-hand combinations. It seems conclusive therefore, that *two-hand combinations are fastest, right-hand combinations next and the left-hand combinations the slowest.*

Another question involved in the general problem of combinations is whether the speed of *two-finger* reactions is greater when the two fingers are on the same hand or on two hands. In our discussion of the fastest combinations, both single and double reactions are considered in the total score. The problem with which we are now concerned deals with *double* reactions only. By converting into percentages all the double reactions (represented in the record sheets by number 17's) and then segregating into three groups, namely two-hand, left-hand and right-hand, a comparison may be made according to Table IX.

For all the subjects except We, double reactions which involve left hand fingers only, are the slowest. For four of the seven subjects, two-finger reactions involving two hands are the fastest and for the remaining three the right hand two-finger reactions are the fastest. Féré[29] concluded that the reaction times for the two hands acting together was greater than either hand acting alone. In no case do the present results agree with this conclusion. On the contrary, reactions with the two hands acting together are faster than the left hand acting alone, and in the majority of cases faster than the right hand acting alone.

---

[29] Féré, Ch., Op. cit., reference footnote 14.

TABLE IX.
Two-finger reactions on a basis of hands.

Subject	Both hands	Left hand	Right hand
We	82	70	61
Wo	50	27	53
Mu	76	74	80
Ga	74	55	65
Mi	78	62	74
Ak	92	83	89
Go	44	22	50

Symmetrical Movements.—It would seem on first consideration that not only should two-hand movements be faster, but that also those two-hand movements which are symmetrical should be the faster. In the ranking of the combinations in the order of speed, the symmetrical combinations, i.e., combinations 10, 29, 38, 47, and 56, do not occur frequently enough to justify the assumption that the symmetry plays any role. In the fastest twenty combinations, combination 10 appears for four subjects, 29 for none, 38 for three, 47 for one, and 56 for four. Out of a possible thirty-five times in the records of the seven subjects, symmetrical combinations appear but twelve times and in such cases as they do occur, it seems to be a matter of more efficient finger rather than the symmetrical two-hand movement.

If the double reactions alone are considered, the average of the 17's for the five symmetrical combinations is considerably less than the average of all two-hand double reactions. For five of the seven subjects and for one of the two remaining subjects the values are exactly the same. There seems to be no reason for concluding that symmetrical movements are faster than those two-finger movements that are not symmetrical, although two-finger movements when the fingers are on opposite hands are faster than two-finger movements when the fingers are on the same hand.

Premature Reactions.—Very little attention need be paid here to responses that preceded the stimulus. In the first place the element of anticipation was practically eliminated by means of the irregular order of presentation and time interval between stimuli. The number of premature responses in these experiments was so small that they were discarded. There were only 12 or 15, and these were found chiefly in the records of one subject.

Woodworth[30] points out that if the stimulus follows at an irregular interval, the reaction is not so short as when the procedure is regular. "If, indeed, the procedure is so regular that the movement of the stimulus can be exactly anticipated, the movement may be made to coincide with the stimulus in time, and the whole character or the experiment thus changed. This result is avoided by varying the preliminary interval within narrow limits." When in addition to the irregular time of the presentation of the stimuli, the order is varied, the subject cannot prepare a preliminary "set" for any one reaction. This setting up or opening up of a particular pathway which gives the impulse a quick passage to the motor nerves, as Woodworth describes it, is not wholly possible. Both fingers must be ready to react as quickly as possible. The difficulty often lies in inhibiting the action in the finger not called upon by the stimulus presented. One subject Ga., after taking 50 or more sets was able to anticipate the order of the stimuli. This subject, however, felt that it was no particular advantage, but rather made the performance more complex than the simple response after the stimulus was given, without knowledge of what it was to be. The fact that the subject could anticipate the order after many series had been performed did not influence the results and conclusions derived, from the experiment, for not until after the whole of the 45 combinations had been taken could this subject even anticipate any of the orders.

Effect of Practise.—The question as to the effect of the repetition of the experiment on the later combinations, in other words the effect of practise, was studied. It might seem that those finger combinations which occurred later in the series have an advantage by this fact, due to the practise acquired by performing the experiment with other finger combinations. The order of presentation of the combinations was such that in the first half of the total number of trials, there were used approximately the same number of right hand combinations as left hand combinations, and about twice as many two-hand combinations. This means that there is a balanced distribution of the combinations, so that the greater speed of the right hand combinations over the left, or the greater speed of the fingers of the right hand over those of the left, cannot be attributed to this factor of practise. In addition four of the subjects were given the combinations in the reverse order from the original four subjects.

---

[30] Ladd and Woodworth, "Physiological Psychology," New York, 1911, page 481.

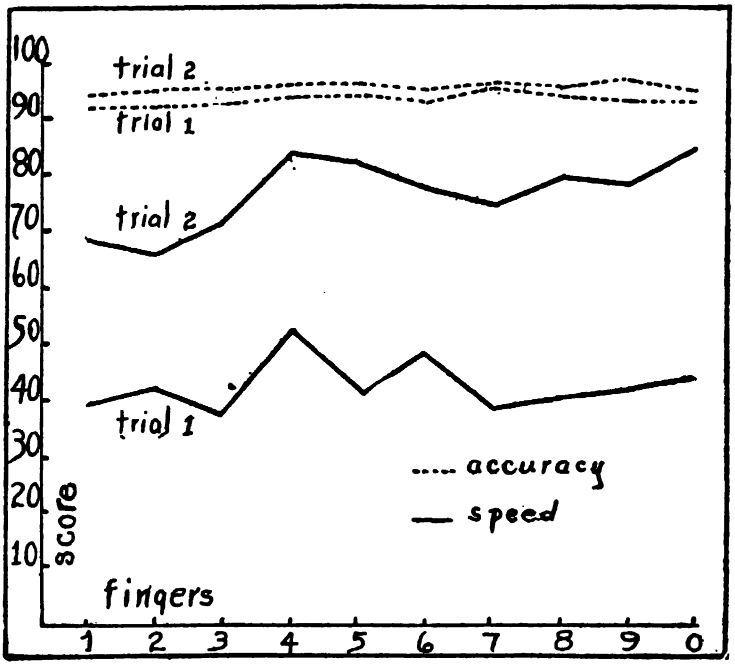
A study in practise effect was carried out by means of a repetition of the entire 45 combinations by one subject Ga., after a period of a few days rest. The result was that in both speed and accuracy trial II give an increase over trial I. Table X shows the relative values according to fingers, for the two trials.

TABLE X.
Trials I and II for subject Ga. (Trial II made after a few days of rest)

Finger	Speed		Accuracy	
	Trial I	Trial II	Trial I	Trial II
1	40	68	92	94
2	42	66	92	95
3	37	72	93	95
4	53	84	94	96
5	42	82	95	96
6	48	78	93	95
7	39	75	95	96
8	40	80	94	95
9	42	79	93	97
0	44	85	93	95

For each finger there is a decided increase after the period of rest. The increase in speed is extremely high. The increase in accuracy is very small, but the fact that there is an increase in each finger when the values representing each finger are derived from averaging 1350 reactions, makes them at least significant. This increase may be in part explicable in terms of the rest period, the so-called "setting" of the habits formed. It is exactly comparable to that which we find in the case of typewriting, telegraphy and other forms of learning experiments. Cattell also concluded that after an interval of some duration (three months) during which no reactions were made, his subjects' times became in general shorter, markedly so in the case of choice reactions.

The more significant facts brought out by the data obtained by the second trial of Ga. are shown upon examination of Graph II. In both trials there are individual finger differences



GRAPH II.—Practise effects. (On basis of repetition of entire series).

and the general shape of the graph is the same for both trials. The increase is a little more in the case of some fingers than in others, however. The similiarity on the basis of accuracy is even more striking. The amount of practise effect may vary with the respective fingers. It is not the purpose of this work to investigate that point, but it is likely that the muscular and nervous structure of the separate fingers is such that one finger will not require the same amount of practice as another in order to obtain a uniform increase in excellency (speed and accuracy). This particular case does not show the marked difference between fingers in either trial, but this was characteristic of the results of Ga's work throughout. As was mentioned earlier, Ga's training had been such as to reduce the difference between fingers which inherently exists, and in addition was a member of a family in which there were several left-handed members and others who showed a tendency towards ambidexterity.

Individual Differences.—Not only between the fingers of a given individual do we find differences, but also in the total speed or accuracy that individuals may attain regardless of the fingers used. These results are best shown in Table XI and Table XII.

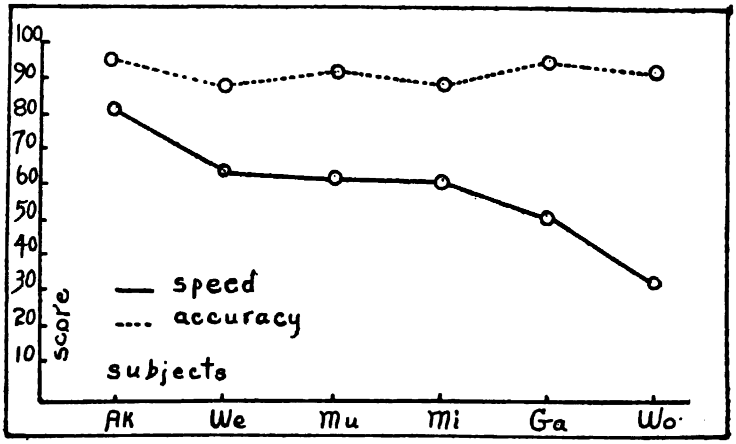
TABLE XI.
Differences between Individuals
(Speed)

Subject	Highest percent attained	Lowest percent attained	Average of all 6750 reactions
We	94 (27)	13 (35)	64
Wo	81 (79)	9 (78)	33
Mu	98 (68)	17 (29)	62
Ga	87 (40)	25 (35, 78)	51
Mi	87 (50)	28 (35)	61
Ak	96 (58)	53 (45)	82

TABLE XII.
Difference between Individuals
(Accuracy)

Subject	Highest percent attained	Lowest percent attained	Average of all 6750 reactions
We	96 (26)	73 (45)	89
Wo	99 (46)	83 (48)	92
Mu	99 (28, 68, 89)	61 (45)	91
Ga	99 (60)	90 (12)	95
Mi	95 (37, 39, 60)	77 (45)	89
Ak	100 (56)	87 (45)	96

The number in parenthesis indicates the finger combination used when the given score was attained.



GRAPH III.—Individual Differences in Total Achievement.

In addition to the preceding records there are the records for subject Go. Go was given the first fifteen combinations with the time set at 400 sigmas. Practically no slow responses were recorded, so that an entire new series was begun, leaving the 15 that had been performed previously, until the last of the set of forty-five combinations. The time used for this series was 300 sigmas. This meant that his average reaction time was enough faster than 400 sigmas to allow even the slower reactions to come above the 400-sigma limit. The records taken from the 300-sigma series show the following:

	Speed	Accuracy
Highest percent attained	72 (47)	93 (19)
Lowest percent attained	7 (14)	63 (90)
Average of all 6750 reactions	31	84

Numbers in parentheses indicate finger combinations.

It is certain that Go. was the fastest of the seven subjects here compared. It is also likely that his reactions are the most accurate, although there is no direct method of comparison. However, 92.6% accuracy with a 300-sigma time limit, is perhaps a better result than those higher figures with the 400-sigma time limit. It does not follow, however, that the person who is the fastest, is also the most accurate. On the basis of the six subjects whose conditions are the same, the fastest subject, Ak, is also the most accurate, but a correlation of the relative positions of the six subjects in speed and accuracy gives a correlation of but .09. Within certain limits the opposite is true. The more time taken in making the responses the more accurate they are likely to be.

There is a considerable difference between individuals in respect to the amount of variation from one combination to another. In speed, the variation ranges from 43 (subject Ak) to 81 (subjects We and Mu). By this is meant the difference between the lowest and highest speeds for the subjects indicated. In accuracy the variation or range is from 9 (subject Ga) to 38 (subject Mu). With one exception, the highest speed was obtained when using the two hands, i.e., one finger on the left hand and one on the right hand. The lowest speed was obtained by three subjects when using the combination 35, and by another when using the combination 45, both of which are left hand combinations involving the thumb. Likewise the greatest accuracy was obtained in every case when a combination involving both hands or two fingers on the right hand was used. The lowest accuracy was attained with the combination 45 by four of the six subjects and with combination 12 by another.

The difference between individuals can best be explained on the basis of the individual differences in reaction tme. This is due in part to individual differences in anatomical and nervous structure. Evidence here is chiefly theoretical and by analogy, but the number of fibres in various muscles involved varies somewhat from individual to individual. Moreover the training of each subject has been unique. The daily practises of no two individuals are exactly alike. Consequently each has developed certain muscles, as well as certain pathways, for his own individual responses.

This fact is evident in a comparison of the results of those subjects whom we might designate as trained and those untrained. Ga and Ak are the two pianists of the group and their results show that the fingers of the practised individuals tend to become more nearly uniform in both speed and accuracy. There is less range of score on the basis of fingers. (Table II and

Table III). There is less difference between hands and between fingers. We hear much talk among teachers of piano technique about developing the speed of the two hands to the same interval, particularly in the two-finger and two-hand movements used in chords. Nevertheless, there has been but slight development of exercises directed toward strengthening the lagging fingers. Little has been known of the actual finger differences, and methods of technique have not been advanced from this angle.

CONCLUSIONS

Briefly stated, the results of these experiments, which include some 70,000 reactions, under as uniform conditions as possible, justify the following conclusions:

1. There are measurable differences between the reactions of the several fingers, first on the basis of speed and second on the basis of accuracy.

2. The fingers of the right hand (at least in right-handed people) are on the average faster than the fingers of the left hand. The fingers of the right hand also excel in accuracy, but the differences in this respect are slight.

3. The finger with which a given finger reacts effects the speed of the given finger. In other words, finger 2, for example, reacts faster when the other finger of the combination is finger 7, than with any other finger. The effect of the combination upon the reaction time of the given finger is relatively uniform for the different subjects. Anatomical structure and common developments due to habits of life are probable explanations for this similarity.

4. Two-finger reactions, at least where there is double stimulation and double reaction, are faster than single-finger reactions, and are also more accurate.

5. Two-hand combinations, i.e., those in which one finger is on the left hand and the other on the right, give faster reaction times than those combinations in which both fingers are on the same hand. Of the twenty combinations, (out of the whole

forty-five) that are the fastest in reaction time, 68 percent of them are two-hand combinations, 26 percent are right-hand combinations, while only 6 percent are left-hand combinations. In opposition to the assertion of Féré, these results justify the conclusion that two-hand reactions are the fastest, right-hand reactions second and left-hand reactions slowest of all, when double reactions are concerned.

6. Practise increases the speed and accuracy of all the fingers. It tends to increase some more than others, thus lessening the difference between fingers. However, after a second performance of the entire forty-five combinations, finger dfferences are still found, *similar* to those originally present.

7. Individuals differ, both in speed and accuracy. There are some individuals who, with unlimited practise will not attain the speed and accuracy which some others show at the initial trial. With very few exceptions, subjects agree that greatest speed is obtained when using two hands, i.e., one finger on each, and the lowest speed is obtained when both fingers used are members of the left hand. The same is true for accuracy. Trained subjects, those having a considerable amount of piano or other similar practise, show less difference between fingers than do the untrained subjects.

GENERAL LIBRARY
NOV 23 1920
UNIV. OF MICH.

Vol. XXVIII
No. 5

PSYCHOLOGICAL REVIEW PUBLICATIONS

Whole No. 127
1920

Psychological Monographs

EDITED BY

JAMES ROWLAND ANGELL, 521 Fifth Avenue, New York.
HOWARD C. WARREN, Princeton University (*Review*)
JOHN B. WATSON, Johns Hopkins University (*J. of Exp. Psychol.*)
SHEPHERD I. FRANZ, Govt. Hosp. for Insane (*Bulletin*) and
MADISON BENTLEY, University of Illinois (*Index*)

The Lag of Visual Sensation in its Relation to Wave Lengths and Intensity of Light

BY

MARION A. BILLS, Ph.D.

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
and LANCASTER, PA.
Agents: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W. C.)
Paris (16, rue de Condé)

Psychological Review Publications

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*J. of Exp. Psych.*)
JAMES R. ANGELL, 522 FIFTH AVENUE, NEW YORK (*Monographs*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*)
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

PSYCHOLOGICAL BULLETIN

containing abstracts, critical reviews, notices of books and articles, psychological news and notes, university notices, and announcements, appears monthly, the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 480 pages. (Suspended temporarily.)

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in May, and may be subscribed for in connection with the periodicals above, or purchased separately.

ANNUAL SUBSCRIPTION RATES

Review: $4.25 (Foreign, $4.50). **Review and Bulletin:** $9.00 (Foreign, $9.50).
Journal: $4.25 (Foreign, $4.50). **Review and Journal:** $8.25 (Foreign, $8.75).
Bulletin: $5.00 (Foreign, $5.25). **Journal and Bulletin:** $9.00 (Foreign, $9.50).
Any one of above with **Index:** $1.25 additional.
Any two of above with **Index:** $1.00 additional.
Review, Bulletin, and Journal: $12.50 (Foreign, $13.25).
Review Bulletin, Journal, and Index: $13.50 (Foreign, $14.25).
Current Numbers: Review or Journal, 80c.; **Bulletin,** 50c.; **Index,** $1.50.
Psychological Monographs: $5.50 per volume (Foreign, $5.80).
Current Issues: prices vary according to size.

THE PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. THE MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages, with a uniform subscription price of $5.50. (Postal Union, $5.80.)

Philosophical Monographs: a series of treatises more philosophical in character.
Library of Genetic Science and Philosophy: a series of bound volumes.

Subscriptions, orders, and business communications may be sent direct to the

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, NEW JERSEY
AND LANCASTER, PA.

FOREIGN AGENTS: G. E. Stechert & Co., London (2 Star Yard, Cary St., W. C.);
Paris (16, rue de Condé)

I. INTRODUCTION

In studying the responses of a sense organ it is often useful to compare them with the responses of physical instruments. In this way not only definite and tangible direction is given to the study but the results obtained are related to principles already well worked out. It has been the writer's purpose to approach the present study, so far as possible, from that point of view. The eye, for example, shows a certain similarity in its responses to well known physical recording instruments; one of the points of similarity is a well marked inertia of response. This is exemplified in the lag which is experienced in the rise of sensation when the stimulus is applied and a persistence of sensation after the stimulus is removed. Both of these phenomena are of great importance to laboratory technique and both should be made the subject of more careful investigation if the eye is to be used with satisfactory results in various phases of laboratory work.

When the eye is used as a measuring instrument it becomes especially important to know its characteristics and peculiarities of response. The chief use of the eye as a measuring instrument, so far as the response which we call visual sensation is concerned, is found in photometry. In photometry a balance or equality of two light intensities is established by the eye at a given point or place, namely, the photometric screen.[1] In establishing this balance in photometry (a) the eye is called upon to respond to a wide range of specific stimuli, and (b) its response to these stimuli is selective both in regard to wave-length and to intensity. That is, the amount of response to a given amount of energy is different for the different wave-lengths of light and the amount of difference varies with the intensity as well as with the wave-length of light. Moreover, with regard to both of these points selectiveness is shown not only in the time required to arouse the maximum of response but in the rate of rise to the maximum or in the lag.

---

[1] This balance, it is scarcely needful to point out, is not in terms of amount of energy of light falling on the screen, but in terms of the power to arouse sensation. This use of the eye in making a balance between two light intensities has often been likened to that of a potentiometer or galvanometer in establishing a balance of potential in an electric circuit by the null method. (See H. E. Ives, Transactions of the Illuminating Engineering Society, 1915, X, p. 103). The analogy, however, is scarcely apt. In establishing a balance of potential in an electric circuit the galvanometer, for example, indicates the balance by giving no response, *i.e.*, it is used in the null method. The adjoining areas of the retina receiving the light from the photometric screen indicate the balance, however, by giving equal responses. That is, the balance obtained could be more accurately compared to the responses obtained in two similar electric circuits by two galvanometers of approximately the same construction, giving equal deflections. A still closer analogy may be found in the case of two similar radiometric instruments which give equal responses.

This brief introductory statement will probably be sufficient to indicate how important it is in evaluating or choosing methods of establishing the photometric balance or in passing on results in any field in which the response of the eye plays a considerable rôle to know in detail the characteristics of the eye as a recording instrument. It is with the more careful determination of one of these characteristics, namely the rate of rise of the response to the maximum for the different wave-lengths of light that this investigation is concerned. Accurate knowledge of this point has not only a strong claim on scientific interest in its own right but it is of fundamental importance to a decision with regard to the correctness of the balance obtained by a method which is at present much used in the photometry of lights differing in color value, namely a method first described by Ogden Rood, and called by him the method of flicker.[2] Equally important also is its bearing on the method of critical frequencies, and on the Talbot-Plateau law, and in fact on all the phenomena arising from giving to the eye in succession light impressions of shorter duration than is required for the sensation to rise to its maximum.[8]

---

[2] The method of flicker belongs to the class of indirect methods and at first was little recognized, because the sensation principles upon which it is based were not understood. Of late, however, a growing tendency has been shown to adopt the method both in the field and in the more precise work of the laboratory for the photometering of lights differing in color value. This has not been because of any scientific demonstration of the soundness of the principle on which the method is based, but on account of the higher sensitivity and precision when the lights employed differ widely in color value than is provided by the older and better established method. Space cannot be taken here to show the connection which the difference in the lag of eye's response to different wave-lengths has on the correctness of the balance established by this method. We may quote briefly, however, from a discussion of this point. "Points are enumerated in the paper appended which raise doubt with regard to the correctness of the photometric balance obtained by the method of flicker. Only one of these is discussed, namely, the

The problem of determining the inertia of response of the eye to different wave-lengths of light presents many points of inter-

---

influence of the time element in the exposure of the eye to the lights to be compared. With regard to this point, it is shown from experimental data (1) that the sensations aroused by lights differing in color value rise to their maximum brightness at different rates; and (2) that the single exposures used in the method of flicker are much shorter than is required for these sensations to rise to their full value. The eye, therefore, is very much under-exposed to its stimulus by the method of flicker. That is, the rate of succession used in the method of flicker is too fast for the single impressions to arouse their maximum effect in sensation and too slow for the successive impressions to add or summate as much as they would need to do to rise to their full value or perhaps even to a higher value than would be given by the individual exposures. Only one other possibility for a correct balance remains,—equality is attained at some value lower than the full value. This can not be assumed, however, without violating well-known laws relating to the factors which influence persistence of vision.

"The principal point of discussion, then, is to what degree it should be held that the difference in lag between the sensations aroused by the single exposures used in the method of flicker is obliterated in a succssion of exposures. Broadly considered, three positions are possible with regard to the point for the rates of succession that are employed in the method of flicker. (1) The difference is not obliterated at all. In this case the photometric balance should deviate from the true balance in direct proportion to the difference in lag for the single exposures. (2) The difference is in part obliterated, but it is still present to a degree which renders the method untenable for precise work. And (3) the difference is entirely obliterated or so nearly so as to be of no practical consequence to the validity of the method. The second is approximately the position taken in this paper. The following evidence is offered in support of this position." (C. E. Ferree and G. Rand: A Preliminary Study of the Deficiencies of the Method of Flicker in the Photometry of Lights Differing in Color. Psychol. Rev., 1915, XXII, pp. 110-163.)

[8] The determination of the time required for sensation to rise to its maximum is also of importance in determining the rate of signalling in case the lights are required to be seen at great distances. An example of this is to be found in the rotating and intermittent lights used in light houses. To have maximum visibility the light should be emitted in any given direction just so long as is required for the sensation aroused by that kind of light to reach its maximum. See "The Perception of Lights of Short Duration at their Range Limits." Blondel and Rey, Transactions of Illuminating Engineering Society, 1912, 7, pp. 625-662.

est. Not all of these can be taken up in the present investigation. It is, in fact, the purpose of the writer to make this investigation only the initial study of a series in which the field will be developed from several points of view. As indicated above, the present study has received its direction largely from current issues in heterochromatic photometry. Because of this the following features will be observed in making the investigation. (a) The determination will be made only in terms of the achromatic or brightness aspect of the sensation. (b) The lights used will be made photometrically equal. This, it is obvious, is essential to the bearing of the investigation on the problem of photometry, for the end point of the photometric observation is equality. The photometric specifications, however, will be supplemented by radiometric specifications. And (c) the determinations will be made for intensities of light selected as far as possible with reference to the photometric problem under consideration.

This being the initial study in a series, the testing and devising of methods will be made an important feature of the work. Up to this time little if any systematic attention has been given by the individual investigators, to a comparative study of methods nor has the control or specification of conditions been such that it is possible to make this comparison from the published work. The following points may be noted with regard to the work that has already been done:

(1) While a number of investigators have made the determinations no two have used methods which are the same in all of the essential details. This has been more nearly done with the direct methods but in these cases no comparison can be made of the results obtained because of the disparity in the experimental technique used, and in the intensity and composition of the lights employed.

(2) No systematic attempt has been made at an equalization of light intensities, either photometric or radiometric and rarely has even an attempt been made at an adequate specification of the light employed.

(3) Some have made the determination in terms of the achro-

matic aspect of the sensation alone, some in terms of the chromatic aspect and some have made both aspects the object of each judgment. Others have made no distinction in the two aspects.

(4) In two cases only have lights of spectrum purity been used; in the remaining cases no attempt has been made to give either a spectro-photometric or a spectro-radiometric specification of the lights employed. Of the two cases in which the stimuli were chosen from the spectrum, a specification in terms of wave-lengths was given in one case; in the other case the part of the spectrum used was roughly indicated in terms of the color of the sensation aroused.

And (5) in most cases no attempt has been made to determine more than the time required for the sensation to reach its maximum, nor were the methods employed devised with any other object in view. Our own investigation, determined partly by the present status of the problem, partly by the special purpose stated above, will comprise the following features: (a) An apparatus will be employed which makes it possible to present the stimuli to the eye in the way that is required by each of the methods that have been used by previous investigators. Thus a comparison of the results obtained by the different methods is not prevented by a disparity of experimental technique. Also by means of this apparatus not only may the lights be taken from a narrow region of the spectrum but the amount used may be specified both photometrically and radiometrically. (b) An attempt will be made to use each of the older methods with the same observer. Also the same amounts and composition of light, will be used, and the general experimental conditions maintained, so that the results obtained by the different methods may be compared. And (c) three new methods will be employed each of which will make it possible directly to compare the different rates of rise of the sensation toward their maximum value, as well as to determine the maximum value itself.

II. HISTORICAL AND CRITICAL

As early as the time of Bacon it was noted that there is an inertia or lag in the response of the eye to light.

> "At in visu (cujus actio est pernicissima) liquet etiam requiri ad eum actuandum momenta certa temporis: idque probatur ex iis, quae propter motus velocitatem non cernuntur; ut ex latione pilae ex sclopeto. Velocior enim est praetervolatio pilae, quam impressio speciei ejus quae deferri poterat ad visum."[4]

Beudant[5] states that an object which moves with extreme rapidity before the eye is not perceived because sensory impressions are not made on the eye instantly. Plateau[6] made the observation that when a square of white paper is passed rapidly before the eye it appears not white but gray. He was also the first to express the belief that color sensation does not come at once to its maximum. Swan[7] noticed "that the light of the sky seen immediately over a ball in its descent through the air, seemed less bright than at those parts of the retina where the action of light had not been interrupted by the passage of the dark body." He conducted some experiments also to determine the intensity of light sensation with short exposures. Subjecting the eye to the action of lights of different intensities for intervals ranging from 1/100 to 1/16 of a second, he found that "colorless lights of different intensities produce like proportions of their total effect on the eye in equal times." He did not directly determine the interval required for the light sensation to come to its maximum, but he estimated it from the results of his experiments with short exposures to be about 1/10 of a second. The task of experimentally determining the interval required for the sensation to come to its maximum was not begun, in fact, until 1868 when Exner, working in the laboratory of Helmholtz, definitely formulated the problem and proposed a method for its solution. In the work thus inaugurated two classes of methods have been employed, the direct and the indirect. Although simpler in form the direct methods have been the later to be used in making the determination. The principles on which these methods are based, were first employed in a crude form by Swan in 1849 in his experiments on the effect on the eye of short exposures of light. They were not developed and applied to the more specific problem, however, until a half century later, by which time most of the work with the indirect methods had been completed. In the order of development, then, the indirect methods came first. In the presentation here, however, this order will be reversed and the direct methods will be considered first. This will be done for two reasons, (a) because the direct methods logically precede the indirect, and (b) because it will be easier to understand why the indirect methods have been resorted to when the difficulties attending the application of the direct methods are made clear.

---

[4] Novum Organum, 1711 lib. ii. Aph. xlvi.

[5] Essai d'un Cours Elementaire et Général des Sciences Physiques; Partie Physique, 1815, p. 480 de la 3 me edition.

[6] Nouveaux Memories de l'Academie Royale des Sciences et Belles Lettres de Bruxelles, 1834, viii, p. 53.

[7] Trans. Roy. Soc. Edinb., 1849, XVI, pp. 581-603.

A. The Direct Methods. A direct method was first applied to the determination of the time required for a sensation to come to its maximum by Charpentier in 1887. Direct methods were later used by Broca and Sulzer in 1903, by Buchner in 1906, and by Berliner in 1907. In the presentation of the work of these men, the chronological order will again be deviated from and the work of Broca and Sulzer will be considered first. This is done because the fundamental principles upon which the direct methods are based, can be presented and discussed more conveniently and comprehensively from their work as a basis than from the work of their predecessors. The reasons which may be offered for this are: (a) although these methods all sustain a close logical relation to the standard method of photometry, namely, the equality of brightness method, Broca and Sulzer have deviated the least from the type in the development of the method and in its application to the detection of lag; and, (b) not only have they used the equality of brightness method more correctly than their predecessors, but they have attacked their problem much more systematically and have used better methods of physical control.

(1) *Broca and Sulzer.* As used by Broca and Sulzer in 1903 the method is as follows. Two surfaces were illuminated by the light, the effect of which on sensation for different lengths of exposure time was to be determined. One of these surfaces was chosen as the standard and the other was compared with it, in a series of judgments of equality. Before beginning each experimental series the two surfaces were made of equal brightness and the photometric value of the equal illumination was determined. During the series of judgments the standard surface was illuminated continuously and the comparison surface for .01, .02, .03, etc. seconds, according to the needs of the problem. With each change in the time of illumination of the comparison surface the intensity of light on the standard surface was altered until the two were judged of equal brightness. The photometric value of the light falling on the standard surface was determined in each case and this was taken as the sensation value of the comparison light for the length of exposure used. From the experiments thus conducted a series of photometric values was obtained, for different intervals of exposure to the comparison light. These values were plotted in the form of a curve which was believed by Broca and Sulzer to show the rate of rise of the sensation to the maximum as well as the maximum itself.

While the method is commendable in its simplicity and is of a type that would naturally appeal to the photometrist it is open to two serious criticisms. (1) The attempt to rate sensation for comparative purposes in terms of photometric units is, it is obvious, fundamentally wrong. For example, a foot-candle may be defined as the amount of light falling on a unit surface at a distance of one foot from a standard candle or a lumen on a square foot; the amount falling on a similar unit of surface at a distance of one-half foot from the standard candle would then be four foot candles. Rated in photometric units the illuminations of the two surfaces would sustain to each other the ratio of 4: 1. The sensations aroused by the two surfaces cannot, however, be said to sustain the ratio of 4: 1. When inspecting the curves of Broca and Sulzer, therefore, it must be borne in mind that the plotting is done in terms of photometric units and not in terms of sensation[8] nor even in just noticeable changes from the zero of sensation. (2) The attempt to employ unmodified the photometric method to determine the effect on the eye of short exposures to the comparison light—all differing in value from each other and from the exposures to the standard light—is, in reality, based upon a grave misconception of the use of the eye as a measuring instrument. In photometry neighboring surfaces of the retina are exposed to the standard and comparison lights. These two surfaces, as already stated, may be likened roughly, to two recording instruments, approximately identical in kind and capable of independent response. If these two lights are of the same intensity and composition and are exposed the same length of time, the same responses will be recorded. If, however, the exposure be for different lengths of time, different responses will be given. Since in the usual practice of photometry the intervals of exposure are of the same length, the time element, so far as the essential correctness of the balance is concerned, may be ruled out of consideration and the two receiving surfaces may be used to equate light intensities at the photometric screen in terms of their power to arouse sensation.[9] If, however, one surface were exposed longer than the other as is the case, for example, in the determination of the rise of sensation, the two surfaces could no longer be used for this purpose, unless indeed the eye were calibrated for the one and the response to the other were rated in terms of the calibrated values.

---

[8] It is not our purpose to raise here the mooted question whether or not it is possible to establish a unit of sensation. We would prefer rather to leave that question in abeyance and to make our expression of the rise of sensation in terms of just noticeable changes from the zero value. We believe nothing more can be done at this time with safety. In doing this, moreover, it is not to be understood that we are subscribing to Fechner's belief that these just noticeable changes, as sensation quantities, are equal in value. That question, also, is left entirely open.

[9] This, of course, is strictly true only when the lights compared are of the same composition. It cannot be said, for example, that a balance made for lights differing in composition would hold for any other intervals of exposures than the ones used, even though the intervals are equal. That is, sensations aroused by lights of different composition neither rise nor decay at the same rate, hence an equality value between such lights is valid only for the interval of time used.

But, it may be said that in the determination of lag, the problem is not the equalization of light intensities, which demands that the lights compared act on the eye for equal lengths of time or that a calibration or correction factor be applied. There is no attempt in this determination at an evaluation of a stimulus by means of the eye. The problem ends with the matching of the standard and comparison sensations. These considerations render it none the less necessary, however, that if the comparison sensations are to be rated from minimum to maximum value the variable influence of the time element on the standard sensations must be eliminated from the experiment. The method must provide a series of representative -standard sensations graded in brightness to which to match the comparison sensations for the different intervals of time. In the failure to provide such a series as a standard, lies the weakness of the method so far as it has been developed up to the present time. The failure of Broca and Sulzer to establish this standard depends in part at least upon their misconception of the temporal course of sensation after it has reached the maximum. In preliminary observations they had found that sensation rises to a maximum, then for a very brief space of time decreases in value. They did not, however, carry the observation far enough to learn that this decay continues and although it takes place more slowly is just as characteristic of the temporal course of sensation as is lag. They assumed that there is a permanent level of sensation, and that the maximum is in reality an over-shooting of this level. The permanent level, they believe, is attained in about two seconds, hence any exposure of the eye to the stimulus for two seconds or longer guarantees a constant response in sensation. The sensation from the standard light could then, they believed, be graded in terms of the stimulus and all that was required in the problem of determination of lag was to expose the comparison light for the required interval and find that intensity of standard light which, when exposed two seconds or more, would give a sensation matching in brightness the sensation given by the comparison light. Indeed, so sure were they of the permanency of the sensation after two seconds of exposure to the stimulus that they made no attempt to have the intervals of exposure equal in the several cases. The only requirement was that these intervals be equal to or longer than two seconds.

In detail the application of the methods was as follows. Determinations were made for white light and for the colors red, green, and blue. In the work with white light the following apparatus and method for making the exposure was used.[10] A white card A was illuminated by a lamp B, enclosed in a lamp-house. This lamp-house was provided in front with a "Blondel" diaphragm D to regulate the amount of light emitted. The light reflected from A is focused by means of a lens O_1, on an episkotister E. Spreading from this image the light is again focused on an opening P_1, in a screen. On this screen adjacent to P_1, is fastened a cardboard P_2, having the same coefficient of reflection as A and of the same size as P_1. This is illuminated by a second lamp B_2 provided with a housing and a Blondel diaphragm similar to B_1. P_2 is made the standard surface and P_1 the comparison surface. The length of time P_1 is illuminated is controlled by the size of the open sector in E. This open sector was rotated at a rate which allowed an interval of one second[11]

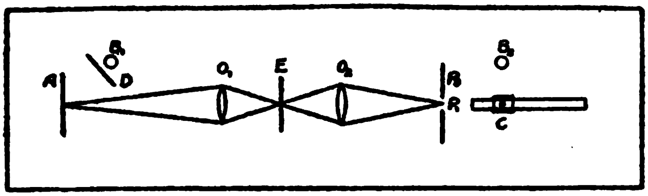


FIGURE 1

---

[10] See "Les fonctions retiniennes en fonction du temps," Annales d'Oculistique, 1904, 131, p. 107.

[11] It is questionable, of course, whether the same value per exposure is given to sensation by a succession of exposures separated by an interval of one second as is given by a single exposure. Broca and Sulzer's own results seem to indicate that there is not the same effect. For example, they say that when the exposures are one second apart the sensation aroused by the first is not so bright as for the succeeding exposures; also that when a longer interval is used between exposures, the sensations aroused are not so bright, until quite a number of impressions have been made, as they are when the interval, one second, is used. Both of these statements seem to indicate that a summation effect of one impression upon the other was present.

to elapse between the successive exposures. P_1 and P_2 were viewed through a tube C.

In the work with colored light a different apparatus was used, although the principle of experimentation was the same.[12] In

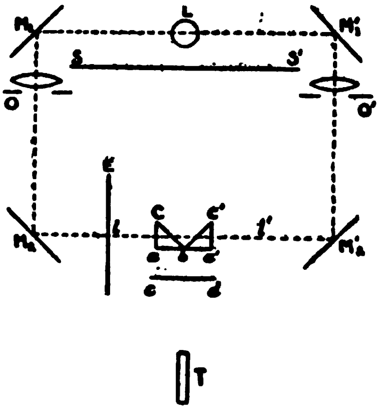


Figure II

general this consists of a source of Light L (acetylene) and two identical optical systems, one of which is made up of mirrors M_1 and M_2, a lens, and the total reflecting prism abc; the other, of the mirrors $M_1{}^1$ and $M_2{}^1$, a lens and the total reflecting prism a′ b c′. The first of these gives the comparison light to the eye; the second the standard light. At E is an episkotister on which the light reflected from the mirror M_2 is focused. By means of this episkotister the time of exposure of the comparison light is regulated. At c—d is placed the filter by means of which the colored light is separated out from the white light. S—S′ is a screen which protects the eyes from direct light from L, and T is a tube through which the standard and comparison surfaces are viewed.

The results obtained in this work were expressed in the form of curves in which the photometric value of the standard light, given in lux, was plotted against that interval of exposure of the comparison light for which the judgment of brightness equality was made at the photometric screen. This gave, by the terms of the method, the photometric rating of the comparison light for different intervals of action on the eye. Figure III is a reproduction of their curves. An inspection of these curves will show among other things that although, as stated earlier in the paper, Broca and Sulzer have made no systematic attempt to equate their stimuli in brightness, they have in all cases given a photometric specification of the lights used. In some cases, moreover, the colors have had equal photometric values. By the phrase "permanent sensation line," they have designated the sensation value for a given light at any time after it has been exposed to the eye for two or more seconds. That this designation is based on a false conception of the temporal course of sensation, scarcely needs again to be pointed out. Sensation does not attain to a constant level at the end of two seconds. It must be remembered, therefore, that the shape of the curves given by Broca and Sulzer depends both upon the rate of exhaustion of the eye by the standard light and the actual rate of rise of sen-

---

[12] "La sensation lumineuse en fonction du temps, pour les lumières colorees." "Journal d'Physiol,' 1904, vi, pp. 55-68. Also "Comptes Rendus des seances de l'Academie des Sciences," 1903, CXXXVII, pp. 944-979.

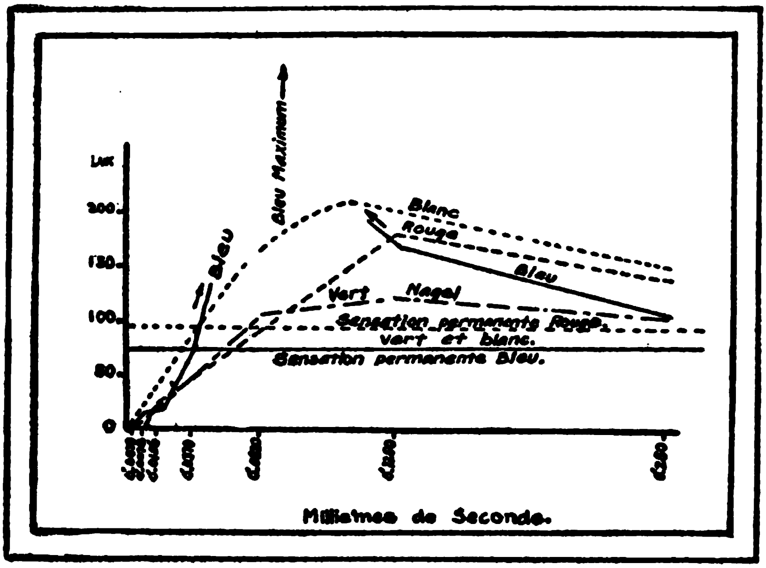


FIGURE III

sation to its maximum. The effect of differences both of wavelength and intensity of light upon the rate of exhaustion of the eye, and of rate of exhaustion upon the results obtained by the method will be taken up later in the discussion of the method.

As used by Büchner and Berliner the direct method was improved in general principle by giving to the standard light a constant interval of exposure.

(2) *Büchner*. The object of Büchner's work[18] was to determine the effect of difference of intensity on the time required for the sensation aroused by white light to come to its maximum, and to plot the rise of sensation to its maximum, under conditions of both light and dark adaptation. The method by means of which his stimuli were presented to the eye was similar to that previously employed by Lough (see this paper p. 19). For work with dark adaptation the arrangement of apparatus was as follows. (See Figure IV.) A milk glass screen M was illuminated from behind by an arc lamp B. In front of this screen were placed, one above the other two slits, S and S′, variable in width. The light emerging from these slits was allowed to spread to a second milk glass screen, F, producing on this screen two continguous bands of light, O and O′. These bands

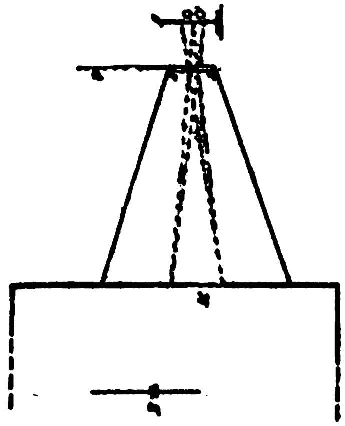


FIGURE IV

---

[18] "Über das Ansteigen der Helligkeitserregung." Psychologische Studien, 1906-1907, II, pp. 1-29.

were viewed directly by the observer. The regulation of the time of exposure of these luminous areas to the eye was accomplished by means of a pendulum provided with two openings, one in front of S and the other in front of S′. The opening in the one which exposed the standard light was kept constant, and the opening in the one which exposed the comparison light was varied. With a given time of exposure of the comparison light, the intensity of the standard light was varied by changing the width of slit S until the two lights appeared equal at the moment of their disappearance. The time of exposure of the comparison light for which the widest value of slit S had to be used was taken as the time required for the comparison light of a given intensity to reach its maximum value. The method was thus similar to that employed by Broca and Sulzer with the exception a) Broca and Sulzer plotted the increase of the standard light required to match the sensation aroused by the comparison light for the different lengths of exposures in terms of photometric units while Büchner plotted these values in terms of slit widths and b) Büchner used a constant interval of exposure of the standard light.

The following values were obtained for the development time. These values, it is stated, are only approximate.[14]

TABLE I

Intensity	*Development-Time*
0.072 M.C.	0.230 Sec.
0.145 M.C.	0.200 Sec.
0.260 M.C.	0.120 Sec.

For the work with light adaptation, a different arrangement of apparatus was used in order that a higher intensity of light might be gotten. Light from the lamp B (Fig. V) was thrown on a translucent screen F′ by two mirrors Sn and Sv. Sn reflected light to the upper half of the screen, Sv to the lower half. The intensity of these images was varied as needed by changing the distance of the mirror Sv from the screen, and by interposing a dark glass (g) between the source and the mirror Sn. The method of regulating the time of exposure and the general principle of working were the same as for the work with dark adaptation. The results obtained are as follows:

---

[14] So far as we can tell from Büchner's report of his work, there was no means of keeping the spots of light, which served as his standard and comparison, of constant area. The vertical dimension was constant, determined by the constant vertical dimension of the opening in the pendulum used to make the exposures, but there was no provision to keep the horizontal dimension from varying with the distance of the screen from the mirrors. In all probability the area of the surface exposed has also an effect on the development-time since, within limits, increase of area stimulated increases the intensity of the sensation aroused. At least, it cannot be assumed, prior to experimentation, that this was a negligible factor.

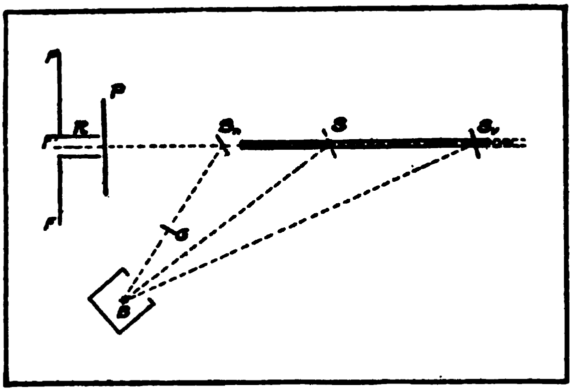


Figure V

Table II

Intensity	*Development-Time*
9 M.C.	.060 Sec.
15.5 M.C.	.070 Sec.
57 M.C.	.033 Sec.

No comparison is made of the time required to arouse the maximum of sensation under conditions of light and dark adaptation with lights of the same intensity. Besides stating that a difference in the intensity of the light used affects the development time, Büchner reports also two other points. (1) The sensation does not rise regularly to its maximum value, (see curves op. cit., pp. 14-24), and (2) the rate of rise is different for different parts of the retina. Only rough experiments were conducted, however, to show the latter point.

(3) *Berliner's*[15] investigation had a two-fold object: (a) a determination of the time required for color sensations to reach their maximum saturation and (b) the time required for them to reach their maximum brightness. The general principle of experimentation was the same as was used by Büchner. The work on the rise of the sensation to its maximum saturation will be described first. Colored lights were obtained by means of pigment papers and gelatine filters. For the work with pigment papers a rotating disc was used made up of sets of variable sectors of colored and gray paper, outer and inner. One of these was made the standard and the other the comparison stimulus. For the comparison stimulus the colored and gray sectors were given a value which would produce, when the disc was rotated, a color of the desired degree of saturation. The proportion of colored and gray sectors in the standard disc was altered until the two were of equal saturation, at the moment of disappearance of both stimuli. The time of exposure to each stimulus was regulated by means of the same kind of pendulum as was used by Büchner. One of the openings in the exposure device carried by the pendulum passed in front of the standard stimulus; the other, in front of the comparison. The eye of the observer was screened from all of the disc but the part in front of which the exposure apparatus passed. From these experiments with pigment papers, green, red, blue, and yellow, Berliner concluded that color sensations all reach their maximum saturation in approximately the same length of time. This time he found to be about 0.300 seconds. In his experiments with the colored gelatine filters the saturation of the standard stimulus was varied to suit the needs of the experiment by rotating in front of one half of the gelatine plate an episkotister, the closed sectors of which were covered with a gray of the brightness of the color. The method of exposing the stimuli and of detecting the development-time was the same as for the pigment papers. Again each of the four colors reached its maximum saturation in approximately 0.300 seconds.

In his determination of the time required for the colors to reach their maximum brightness only two colors were used. These colors were obtained by means of gelatine filters. The experimental procedure was the same as was employed for the determination of the time required for the colors to reach their maximum saturation, with the exception that the closed sectors of the episkotister used to vary the brightness of the standard stimulus were made black instead of the gray of the brightness of the color. The maximum brightness of each of these two colors was attained in approximately 0.130 seconds. Berliner also mentions the irregularities in the rates of development of the color sensations. This was especially true of their rise in saturation.

---

[15] "Der Anstieg der reinen Farbenerregung im Sehorgan" Psychologische Studien, 1907, III, pp. 91-155.

(4) *Charpentier.* Charpentier[16] determined the effect of difference of intensity on the time required for white light to produce its maximum effect in sensation. Working with four intensities of light, he derived the law that the time required for the sensation to reach its maximum value varies inversely as the fourth root of the intensity of the light. He does not fully describe his method of working; but, so far as can be determined from the description given, it is considerably cruder than that employed by Broca and Sulzer, Büchner, or Berliner.

(5) *Lough.*[17] Lough, 1896, worked also with white lights of different intensities. With regard to the effect of intensity on the rate of rise of sensation he came to the following conclusions. (1) The time required for the sensation to reach its maximum varies with the intensity of light. An increase in the intensity of light decreases the time required for the sensation to come to its full value. (2) For intervals of exposure less than is needed to bring the sensation to its maximum value, the intensity of sensation varies directly as the time of exposure. The effect of intensity on the time required for the sensation to come to its maximum is shown in the following table. The in-

---

[16] Recherches sur la persistance des impressions rétiniennes et sur les excitations lumineuses de courte durée. Archives d'Ophtalmologie, 1890, X, pp. 107-135.

[17] "The Relations of Intensity to Duration of Stimulation in our Sensation of Light," Psychological Review, III, 1896, pp. 489-492.

tensities of the lights used are not specified in standard units. Only ratios are given in an arbitrary scale.[18]

TABLE III

Intensity of Light	*Time of maximum effect*
1	0.148 Sec.
4	0.110 Sec.
16	0.100 Sec.
64	0.085 Sec.
256	0.090 Sec.

In making these experiments Lough used the following apparatus. The light employed in making the determinations came from two standard candles, a and b, (See Fig. VI) which could be slid back and forth on two wooden arms, AB and BC, one slightly above the other. The light was reflected through a slit E into the observation tube F by means of a mat white reflector a′ (coefficient of reflection not given) and the light from b by means of a similar reflector b′, which is slightly above or below a′. Slit E is one centimeter wide and four centimeters high. The upper half of this slit is illuminated by the light from a′, and the lower half, from b′. The eye is protected from the direct light from a and b by means of the tube F and the screens, G and G′. Between a′ and b′ and the slit E swings the exposure apparatus D, which is a pendulum carrying a mat black screen. This screen contains two open sectors, S and S′, not shown in the figure, one twice as broad as the other. Lough describes the method of detecting the maximum as follows:

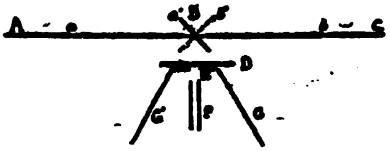


FIGURE VI

"In order to obtain the time for the maximum effect, the lamps were placed so that a′ and b′ gave sensations of equal intensity and S and S′ were then adjusted to the relation of 1 : 2. When now the pendulum was allowed to swing through a small arc the two reflectors seemed equal, but as the amplitude of the swing increased, a point was soon reached where a′ appeared just preceptibly darker than b′. This marks the point where a′ fails to produce its maximum effect. With openings and lamps adjusted as before, the pendulum was now given a much larger swing. This caused a′ to appear much less intense, while b′ retained its former intensity. The amplitude of swing was gradually decreased; with this a′ becomes gradually more intense until it finally becomes equal to b′; after this no further change will take place. This also marks the point of maximum effect for a′."

---

[18] The unit of intensity used by Lough, worked out roughly from the data given by himself, is 0.097 meter candle.

(6) *Martius.* The form in which Martius[19] employed the direct method of making the determination seems also to be open to question. The surfaces to be compared filled the visual fields of two telescopes. The standard light illuminated the field continuously, while the time of exposure of the comparison field could be varied. The observer looked first at the standard light, then quickly moved his eye to the other telescope and viewed the comparison light. The maximum was determined by a series of observations similar in principle apparently to those made by Broca and Sulzer. Two conspicuous errors were involved in Martius' method of making the determinations. (1) Even less adequate provision was made for standardizing the sensation which served as standard than was made, for example, by Büchner and Berliner. And, (2) a serious time-error was involved in making the comparison of the two fields. That is the two sensations were not compared directly. One was compared with the variable or changing memory of the other. The seriousness of this error will be obvious to any one who is familiar with the difficulty of making accurate and reproducible judgments for such short intervals of exposures, even when they are made under the most favorable conditions. The exposure apparatus employed by Martius is somewhat elaborate. A schematic representation of this apparatus, drawn by the author, from his description is shown in Figure VII.

The discs D and M, are connected by a transmission gear in

---

[19] "Über die Dauer der Lichtempfindungen" Beitrage zur Psychologie und Philosophie, 1905, I, pp. 275-366.

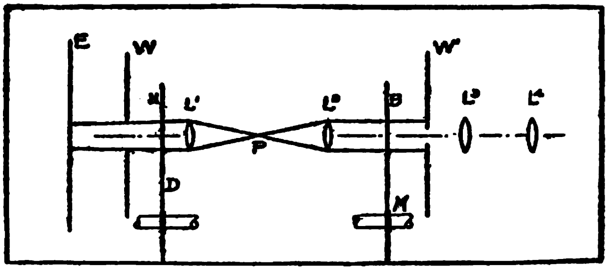


FIGURE VII

such a manner that they may be rotated at different rates. These discs allow the light to pass from the source to the eye, only when the openings, N. and B, come at the same time into the line of sight. The times of exposures and the time between exposures can be reckoned from the relative positions of the openings, N and B, and the speed of rotation of the two discs. The light is presented to the eye in the following manner. An illuminated mat surface, E, serves as the source of light. The light from this passes through an opening in a screen W, and from there through the lens L′ and focuses at P. From P it spreads to the lens, L^2, and is rendered parallel, passing just through a slit in screen W′ and then through the telescope lenses, L^3 and L^4, to the eye. At each exposure the eye viewed the field of the telescope uniformly filled with light. Two of these apparatus were provided, side by side, one to give the standard stimulus and one the comparison. In either, the exposure discs could be rotated if desired. They were rotated, however, only in giving the comparison stimulus. In giving the standard stimulus the light was allowed to come through to the eye continuously. The apparatus used by Martius may be criticised in two regards. (1) The arrangement was such that the standard and comparison lights were presented to the eye successively rather than simultaneously which, as has already been noted, introduced a serious time-error, and (2) the position of the exposure discs in the path of light were not such as to expose or to cut off the light sharply. To accomplish this the light should have been focused in the plane of the discs.

His results agree in general with those of the other observers, in that he found that the more intense was the light, the shorter was the time necessary for it to produce the maximum response. The results are given in the following table. The intensities of light used are specified only in terms of ratios in an arbitrary scale.

TABLE IV

Intensity	*Time required to produce maximum response*
1	0.013 Sec.
1/2	0.039 Sec.
1/4	0.053 Sec.
1/8	0.066 Sec.
1/16	0.078 Sec.
1/32	0.093 Sec.

Martius also carried out a few rough experiments with colored lights, coming to the following conclusion. "If the colors rank from bright to dark in the following order, yellow, green, blue, red, they rise to their maximum in the order yellow, blue, green, red." He gives the appended table.

TABLE V

Color	*Time required to produce maximum response*
Yellow	0.020 Sec.
Blue	0.036 Sec.
Green	0.048 Sec.
Red	0.090 Sec.

B. The Indirect Methods. (1) *Exner*[20] working in the laboratory of Helmholtz, was the first to take up the investigation of the response of the retina to lights of short duration by an indirect method of determination. His method was based on the following principle.[21] If the stimulus for a light sensation is cut off before it has aroused its maximum response in sensation, the sensation will continue in its original brightness, *i.e.* there will be a positive after image. When, however, the stimulus is cut off after it has produced its maximum effect, the sensation will tend to continue in the reversed brightness, *i.e.*, there will be a negative after image. Now, if two adjacent parts of the retina are stimulated by lights of the same intensity, but one excitation is begun slightly before the other, two sensations will result, the course of which may be represented by the curves in Figure VIII. From these curves, granting the principle previously stated, it will be seen that if the two stimuli are interrupted simultaneously before either has reached its maximum (represented in Fig. VIII by m, n, f) both should give positive after images but these after images should be of different brightnesses. That is, the after image of the stimulus, the exposure of which was begun first, will be the brighter. If they are interrupted after both have passed their maximum (represented in Fig. VIII by p, r, e) both should give negative after images, which should also differ in brightness. There is a time, however, at which they may be interrupted (represented in Fig. VIII by c, h) when the result should be two positive after images of equal brightness. The sensation aroused by this length of exposure is, Exner believes, the maximum for the light first exposed. The following comments may be made both on the principle on which the method is based and on the limits of application of the method to the problem in question. (1) It can scarcely be demonstrated within the limits of accuracy required for the method that an under-exposed and a fully exposed eye give positive after responses and an eye barely over-exposed gives only a negative response. The assumption is based on a very general consideration of the shape of the intensity curve for sensation—that is, the sensation rises to its maximum then begins to decay if the stimulus is prolonged—and on Helmholtz's theory of the correlation of the negative after image with the

---

[20] Über die zu einer Gesichtswahrnehmung Nohtige Zeit." Sitzungsberichte der Kaiserlichen Academie der Wissenschaften, 1868, LVIII, pp. 601-631.

[21] Helmholtz apparently saw the possibility of a defect in this principle. Handbuch der Physiologischen Optik, 1896, pp. 513-514.

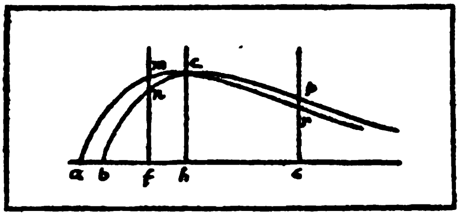


Figure VIII

loss of sensitivity and the decay of sensation. (2) The general principle of detecting the maximum by two exposures, one beginning slightly after the other could be used quite as readily, were the judgments made of the sensations aroused during the exposures instead of the after-responses. There seems in fact even no theoretical justification for making the judgment of the after-images unless it is believed that the brightness reversal which according to the assumption should occur as soon as the point of maximum intensity is passed for the sensation first aroused, would make a greater and, therefore, a more readily detectable difference between the after-images of the two sensations than would be obtained between the two sensations themselves. In contradiction to this, however, Kunkel, the next to take up the work of detecting the time of maximum response, declared the judgment of the after-images to be impossible and used the method with three revisions, one of which was that all judgments were made of the sensations directly aroused by the two exposures. (3) The sensation aroused by the light first exposed is the sensation whose maximum intensity is being determined. Now, if the second exposure is begun too long after the first, the judgment of equality will be made after the sensation aroused by the first exposed light has reached its maximum. If, on the other hand, it is begun too soon after the first, the two sensation curves will too nearly coincide and the judgment of equality will be made for too great a part of the two curves, when theoretically it should be made at a given point in the curve or, at least, for a very small segment of the curve. That is, by the first procedure, greater sensitivity is obtained in detecting the equality point, but the equality point does not coincide with the point of maximum intensity; while, by the second procedure, a lesser sensitivity is attained in detecting the equality point, but the point of maximum intensity lies within the segment judged equal. Obviously a successful application of the method should come from a proper compromise between the two procedures, or the choice of the proper interval of time between the beginning of the exposure of the eye to the first and second stimulus. Both exposures end at the same time, hence, once this interval between the two exposures is chosen, the two are varied by equal amounts until the judgment of equality is made.[22] (4) At best the method can be used only for obtaining the exposure time required to give the maximum intensity of sensation. The relative sensation values of exposure-times less than this can not be determined directly. But once having obtained by Exner's method, the maxima for a series of sensations graded in intensity, the curve for a third sensation, the maximum intensity and the time of development of which is equal to or less than the last one of the graded series, might be determined by the method of direct comparison or equality of brightness. This combination of the two methods, however, has not yet been made.

Exner used only white light in making his determinations. Four intensities which bore the ratio to each other of 1, 2, 4, and 8 were used. The intervals necessary for these different intensities to have maxmium effect in sensation ranged from 0.287 to 0.118 seconds. With reference to the effect of intensity of stimulation on the time required for sensation to reach its maximum he draws the following general conclusion:

> "Wenn die Reizungs-Intensitäten in geometrischer Progression wachsen, so nehmen die Zeiten, die zwischen Beginn der Reizung und ihrer höchsten Intensität verlaufen, in arithmetischer Progression ab."

(2) *Lamansky*. Lamansky[23] was the next to take up the work of determining by an indirect method the time required for the visual sensation to rise to its maximum. He gives in the article cited an extremely brief description of his work. No account of the method used is given further than the statement that it was the same as used by Exner. Pigment papers, white and colored, were used as stimuli. A detailed statement of results is not given. The general conclusion is drawn that the

---

[22] The above comment is perhaps more strictly applicable to the judgment of the sensations than to the judgment of the after-images. It is somewhat difficult to say *a priori* just what the situation would be in case the judgment were made of the after-images.

[23] "Über die Grenzen der Empfindlichkeit des Auges für Spectralfarben," Archiv. für Ophthalmologie, 1871, XVII, pp. 123-132.

color sensations rise to their maxima at different rates but the only statement he makes with regard to the order of rapidity of rise for the different colors is that it is the slowest for red.

(3) *Kunkel.* Three years later the problem was taken up anew by Kunkel.[24] He is also generally accredited with having used Exner's method in a modified form. The modifications made by him, however, were rather fundamental. He repudiated Exner's method in the following regards. (1) His observers were utterly unable to make the comparisons needed in terms of the after-response. And (2) he denied the possibility of determining more than a rough approximation of the time required for the sensation to reach its maximum, when equal intensities of lights were used in conjunction with a constant difference of exposure time, as is required by Exner's method. In devising his own method he begins with the assumption that the more intense is the light, the more quickly will the sensation aroused by it rise toward the maximum.[25] With this assumption as a basis two points of revision are made. (a) The lights used shall be of different intensities. One must be a great deal stronger than the other; and, if the method is to accomplish its purpose, the sensation aroused by the stronger light must, as stated above, rise more rapidly toward its maximum than the sensation aroused by the weaker light. The intervals of exposure of the two lights must be so regulated, however, that the sensation aroused by the comparison light reaches its maximum before the maximum is reached by the sensation aroused by the standard light. And (b) a variable and not a constant difference in the time of exposure to the two lights shall be employed in each series of determinations. In detail the method is as follows. The apparatus is so arranged that the exposure to the standard and comparison lights end at the same time. A given very small interval of time is chosen for the comparison light. Then a time of exposure is found for the standard light

---

[24] "Über die Abhängigkeit der Farbenempfindung von der Zeit," Archiv. für Physiologie (Pflüger) IX, pp. 197-220, 1874, Bd. IX, pp. 197-220.

[25] Exner had found this to be true for white light; Kunkel assumes it also for colored light. Colored light alone was used in his experiments.

which causes it just to match in apparent brightness the comparison light. This procedure is repeated for a graded series of exposure times for the comparison light. From this series the time required for the comparison light to arouse its maximum response in sensation is selected in accordance with the following principle. The exposure times of the standard light are so regulated that the sensation aroused by it would, if the stimulation were allowed to continue, reach its maximum considerably later than the sensation aroused by the comparison light. However, the stimulation by both lights is cut off before the sensation aroused by the standard light reaches its maximum. For the series of observations under consideration, then, it may safely be assumed that the longer the exposure to the standard light, the stronger is the sensation aroused by it. In order to determine the time of exposure required to arouse the maximum value of sensation, it becomes necessary, therefore, only to pick out from the series that value of sensation which is matched by the sensation aroused by the longest exposure to the standard light to be found in the series. The method can be further elucidated, perhaps, by a graphic illustration such as is given in Figure IX.

In this figure the temporal courses of the sensations aroused by the standard and comparison lights are represented by curves in

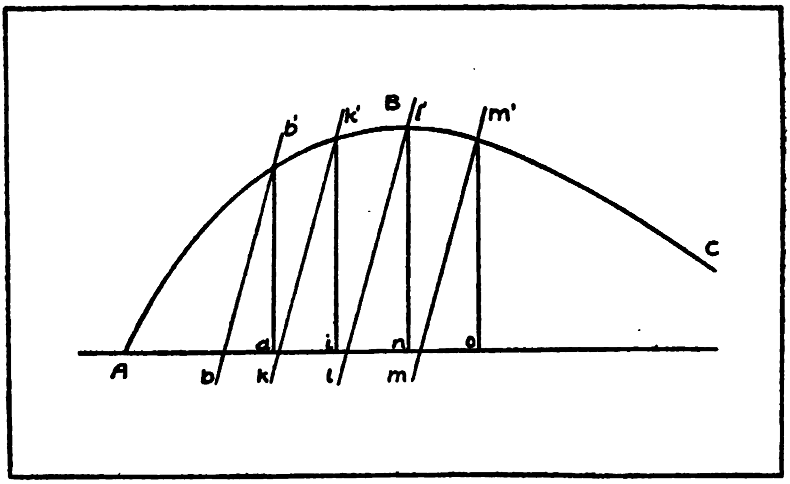


FIGURE IX

which the intensity of sensation is plotted against the time of exposure of the lights. The general course of the comparison sensation is represented by the curve A, B, C; the course of the standard sensation is represented by the curves bb'kk'll' and mm', the times at which the exposure of both lights is cut off, for the cases considered, by the points, d, i, n, and o; and the lengths of the exposure of the two lights, standard and comparison respectively, by the lines bd and Ad, ki and Ai, ln and An, mo and Ao. Since b', k', l', and m' are common to the two curves and are points which represent equal intensities of the two sensations, and both ki, and ln, and mo are the times of exposure required to bring the standard sensations to these values; then if in comparing the values bd, ki, ln, and mo, it is found that ln, has the greatest value but only slightly greater than ki, or Mo, An may be taken as the time required for the comparison sensation to attain its maximum. From a consideration of this method it is obvious that nothing more than a determination of the time required for a sensation to come to its maximum can be made with any degree of accuracy, although Kunkel has in a rough way plotted the rise of the visual sensations. (See Figure X.) He did this by assuming that the rise of the sensation for the more intense stimulus could be represented approximately by a straight line; and that, accordingly, the excitation caused by it may be considered proportional to the length of exposure. He also contends that the time required for a sensation to come to its maximum can be determined with more precision by his revision of the method than by the method in its original form; for, the difference in the pitch of the curves being much greater in the former than in the latter case, not so great a change in exposure time is required to make a noticeable change in sensation. That is by using lights widely different in intensity and a variable rather than a constant exposure time, he has succeeded, he believes, in giving to Exner's method a greater sensitivity than was attained in the form in which it was employed by Exner himself.

Kunkel worked with colored light alone,—a red, green and blue chosen from the spectrum. The range of wave-lengths, however, was not specified. The spectroscope used by him was apparently not calibrated for wave-lengths and his only means of reproducing his stimulus from observation to observation was his judgment of similarity of color tone. Two intensities of lights were used for red and blue, and three for green. No specification is given of the intensities used, nor can they be determined from any data given by him. He shows his results for these intensities in the form of curves, given in Figure X.[26]

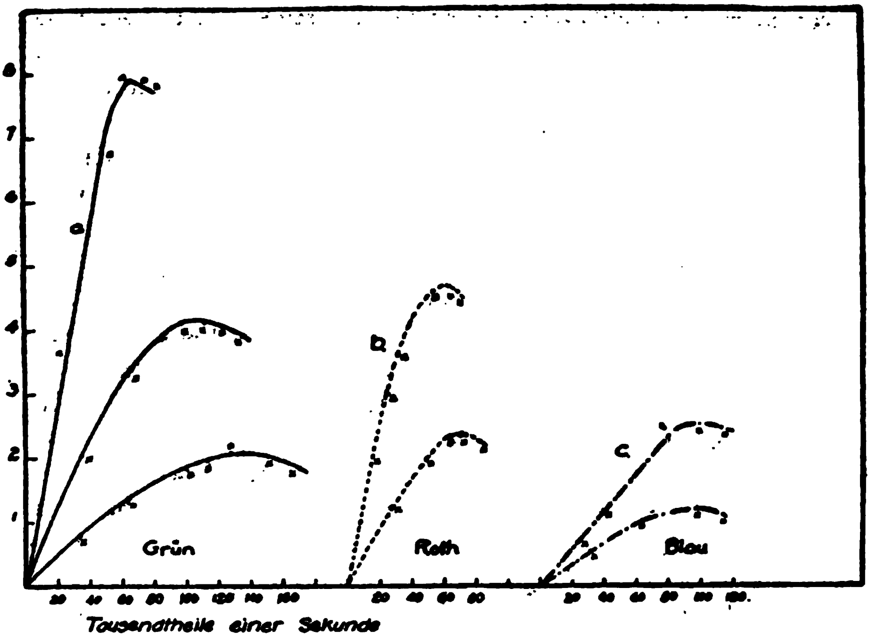


FIGURE X

A crude attempt was made to get comparative results with lights of equal brightnesses. The lights were not equated by photometric comparison, but he assumed that the green of his spectrum was twice as bright as the red and four times as bright as the blue, and made reductions of the intensity of the red and green to accord with this rough estimation.

---

[26] The values shown in these curves for the visual sensation were obtained in the following manner. The light to be used as a standard was made of sufficient intensity so that its brightness might be considered to increase in direct proportion to its time of exposure. The comparison light was then rated for different exposure times in terms of the exposure time of the standard light which caused the two stimuli to be judged equal. The ordinates of the curves represent the exposure times of the standard light—plotted to an arbitrary scale.

(4) *Dürr.*[27] Unable to get satisfactory results by the methods of either Exner or Kunkel, Dürr attempted to find still another method of varying the factors involved, in a way that would suit the purpose of the problem. The method he devised may be considered a modification of Kunkel's method. The following points of similarity and difference may be noted between the two methods. Like Kunkel, Dürr used a variable time of exposure to the two lights, and a variable difference in the time of beginning these exposures. He also used lights differing widely in intensity, and made the stronger the standard, by means of which the maximum value of the sensation aroused by the weaker was detected. Unlike Kunkel, however, (1) he varied the length of the two exposures and the difference in the time of beginning these exposures in such a way that the comparison sensation became equal to the standard after the latter sensation had reached its maximum value, instead of before; and (2) he determined from his results the time required for the comparison sensation to reach its maximum value in accordance with the following principle: In a series of observations in which the variations are made as described above, there is for a given intensity of light one length of exposure to the comparison light that will arouse a sensation which can be judged equal only once in its course to the sensation aroused by the standard light, and that is the time required for the comparison light to arouse its maximum response in sensation. This principle may perhaps be made clear by a consideration of the curves shown in Figure XI.

In this figure, curve A B C, represents the intensity curve of the standard sensation, A′B′C′, the curve of the comparison sensation; and AA′, the interval of time between the beginning of the exposures of the standard and comparison lights. Were the exposures made to end at aa′, cc′ or at any other time than

---

[27] "Über das Ansteigen der Netzhauterregungen," Wundt Phil. Studien, 1901-1903, XVIII, pp. 215-273.

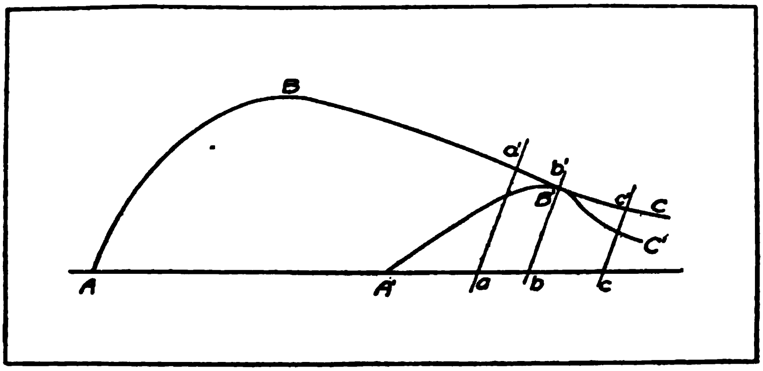


FIGURE XI

is represented by bb′, the comparison light would be judged darker than the standard. Were the time AA′ made shorter, the comparison light would never be judged equal to the standard; and were it made longer the comparison light would twice be judged equal to the standard, and a part of the time stronger and a part weaker. That is, the curve A′B′C′ would rise above the curve A B C and would cut it, therefore, in two places. This would happen also were the time AA′ to remain the same and a stronger light be used for the comparison light. How difficult the method is of a systematic application must be obvious even with this brief consideration of the possibilities. The success of the determination depends upon selecting just the right relation between the three variables,—the ratio of intensity of standard to comparison light, the time from the beginning of the first light to the beginning of the second light, and the length of exposure to the two lights. If this is not done, the curve A′B′C′ will not touch the curve A B C at one point only; and the time required for the comparison sensation to reach its maximum value cannot be determined. Our own attempts to use the method have proved very discouraging, and our verdict is that it is the least feasible of any of the methods that have yet been described.

Using the method described above Dürr obtained results which disagree with those of most other investigators. His conclusions were threefold. (a) The time required for visual sensation to reach its maximum is independent of the intensity of the light used, (b) it is longer for colored than for white light, and (c) it is the same for all of the colors. According to his results this time is, for white light for all intensities used, approximately 0.260 seconds; and for colored lights, about 0.541 seconds. A comparison of his results with those of other investigators shows that even the few who agree with him in some points, do not agree with him in all. Swan alone concurs with him in the belief that the development-time for white light is independent of the intensity of the light used; and Swan's results, it will be remembered, were not directly determined, but computed from experiments with short exposure times. Swan worked with white light alone and found for the development-time a value of about one-half that assigned to it by Dürr. MacDougall agrees with Dürr in that he believes that colors of equal brightness rise to their maximum at the same rate; but he maintains, on the other hand, that the rate of rise sustains a direct relation to the intensity of light used. His value for the development-time is about one-fifth of that obtained by Dürr.

The radical disagreement of Dürr's results with those of other investigators who have used methods differing from his and who have not disagreed so much among themselves, leads one strongly to suspect that the disagreement is largely the result of his method of working. It was our intention to use his method along with the others for the sake of finding out more definitely whether the type of discrepancy shown in his results is characteristic of the method or a peculiarity of the observer. Because of the tediousness of the method, however, and the more or less unsystematic way in which it must be applied, our work with it was confined to one intensity and one color of light. This gave us the opportunity of comparing our results with Dürr's in one regard only, namely, the order of magnitude of the value gotten for the development-time. The effect of wave-length and intensity could not, for example, be compared. With regard to the order of magnitude of the development-time our results agreed with Dürr's. That is, the value of the development time was much greater than was obtained by the other methods. With regard to the causes of this difference in the results obtained by Dürr and other investigators we have only the most casual suggestions to make. (1) It seemed possible that the judgment of the sensations might be influenced by the after-response and that this might lead to the overestimation of the development-time. That is, the standard light is so much stronger than the comparison light and is exposed so much longer that the tendency would be to give a much stronger positive after-effect than is given by the comparison light. Within limits the tendency to do this would possibly decrease with increase in the exposure time to both lights for the increment would doubtless have more effect on the after-response when applied to the relatively short exposure of the comparison light than to the longer exposure of the standard light; and this would exert a tendency to lessen whatever influence the difference of after-response might have on judgment.[28] However, that the after-response does in all probability influence the judgment was shown by decreasing its possibility of occurring and noting the results. This was accomplished by preventing the stimulus-light from being succeeded by the usual dark field produced by cutting off the light from the stimulus box,—a dark field being the most favorable possible for the quick development and brilliancy of the positive after-image. By means of a white mat surface fastened to the exposure disc with its forward edge flush with the edge of the closed sector which cuts off the stimulus light and a small electric lamp the circuit of which was closed at the instant the stimulus light was cut off, the stimulus surface and the interior of the stimulus box was illuminated by white light immediately succeeding the cutting off of the stimulus light. That is, the disappearance of the stimulus was followed by a light rather than a dark field and the positive after-image was not given an opportunity to develop. The effect of this was to lessen very much the determination made of the development-time, and to bring it more nearly into agreement, therefore, with the values obtained by other methods. (2) From the ratios given by Dürr of the intensities of his comparison and standard lights it seems hardly possible that the two could have been judged equal at the end of the time given by Dürr. That is, this ratio was 27 to 4, for example, in one case, and 23 to 4 in another. The time for which the standard light was exposed in each case was about 1.7 seconds. For the comparison light to have been judged equal to it would have necessitated that the intensity of the sensation aroused by the standard light must have as a result of adaptation or exhaustion sunk in less than 2 seconds to a value equal to that given by a light of approximately one sixth or one seventh its intensity. From all that is known of the rate of loss of sensitivity due to exhaustion this seems hardly possible. There is, of course, the possibility that in the adaptation or exhaustion experiments which in most cases have not been done in connection with experiments for the determination of the development-time, account has not been taken of the rapid fall in intensity that occurs just after the maximum time has been reached, and that, this loss, due to exhaustion in a given interval of time, has therefore been underestimated. Such a possibility is, however, ruled out by the results of Broca and Sulzer (See Fig. III) and of the author using the method employed by them (see Table VIII and Fig. XIII). Remembering that the adaptation-time of Broca and Sulzer to the standard light was at least two seconds and that the author's adaptation time was six seconds, it will be seen that here we never have a loss due to exhaustion for two or six seconds greater than that given by decreasing the light one half, except perhaps in the case of blue and white for the highest intensity of light used. (3) Dürr does not give a specification of his lights, either photometrical or radiometrical; apparently, however, the comparison lights used by Dürr were of very low intensity. We judge this to be the case from two facts: (a) the large ratio between the standard and comparison lights, and (b) his statement that the comparison lights were of such low intensities that there was not enough color present to make the photometric comparison with a white light difficult by the equality of brightness method. Whether or not the low intensity of the comparison light sustains any causal relation to the difference in the results obtained by him as compared with others we are not at present able to state. It can be affirmed, however, that the very low intensities of his comparison lights and the high intensities of his standard lights would affect the relative rate at which the curve A′B′C′, after its highest point has been reached, falls away from the curve A B C. This should have a bearing at least on the sensitivity of the method, for if the maximum point is to be located with precision, curve A′B′C′ should after its brightest point has been reached fall away sharply from ABC for upon this difference depends the amount by which the exposure time must be changed before the comparison sensation becomes noticeably less intensive than the standard sensation.

---

[28] The only study of the positive after-image which the author has been able to find, where the lights and exposures employed are at all comparable with those used in the present experiment, was made by Franz in 1901, (Psy. Rev. Mon. Supp., No. 3). He gives the following data. An exposure of 1 sec. arouses a positive after-image in 100 percent of the cases, one of 1/10 sec. in 97 percent of cases; one of 1/100 sec., in 82 percent of the cases; and one of 1/1000 sec., in only 19 percent of the cases. If we can assume that the factors which affect the frequency with which the after image occurs similarly affect the intensity of those which occur, we conclude from this data that an increase in a very short exposure to the light will have more effect than an increase in a longer exposure, within the range of exposures used by Franz. We can give this assumption no other warrant, however, than its plausibility. In this same article Franz shows that the effect of decreasing the intensity of the stimulus is to produce at first a slow and later a more rapid decrease in the frequency with which after-images occur. This fact will be referred to later in another connection.

(5) *MacDougall.* So far the methods discussed have conformed to two general types,—the method of direct comparison or equality of brightness, and the method of Exner with its modifications by Kunkel and Dürr. MacDougall[29] in 1904 introduces a third type. Like Exner's his method may be classed as an indirect method, and was used only for finding one point in the intensity curve, namely, the point of maximum intensity. It differs from Exner's method, however, not only in the means of detecting the point of maximum intensity, but in the manner of making the judgment.

---

[29] "The Variation of the Intensity of Visual Sensations with the Duration of the Stimulus." British Journal of Psychology, 1904, I, pp. 151-189.

MacDougall gives two reasons for attempting to modify the manner of making the judgment. The first is a wish to avoid the effect of the contrast which is present between stimuli simultaneously presented. It is difficult, however, to get his point of view in this regard, for the only effect of simultaneous contrast is to increase the sensitivity of the judgment and therefore to lower the actual amount of change that is needed in the stimulus to produce a just noticeable change in sensation. That is, since stimulus A differs from stimulus B only in brightness not in color value the only chance for contrast will be in the brightness aspect of the sensation. The effect, therefore, will be only to increase the effect on sensation of whatever difference there is in the stimulus and thus render small changes more readily detectable. In fact, it is just this condition that is striven for in the construction of the better class of photometer heads in order to increase the precision and sensitivity of the judgment. The evolution of the photometer head for the equality of brightness method has in fact consisted largely of the development of this feature alone. In this connection, compare, for example, the precision and accuracy of judgment with the type of head used in the Rumford photometer, in which the fields judged are so widely separated as to be removed from all contrast effect on each other, with that of the most modern type of Lummer-Brodhun photometer, in which the fields to be judged are so arranged as to give a maximum of contrast effect of the two fields on each other. MacDougall's second reason for wanting to change the method of making the judgment was because of his belief that it was unnecessarily difficult. The judgment of two stimuli simultaneously presented for so short a time he thought could be made with less accuracy than if the stimuli were presented successively at a favorable rate for a number of times. His apparatus for carrying out this method of making the judgments consisted of a large wooden disc with open sectors diametrically opposed to each other, the size of which could be varied independently to stimulate the eye for different lengths of time. This disc was rotated in front of a milk glass plate which was uniformly illuminated and gave the stimuli used in making the determinations. During one rotation of the disc the eye was thus exposed to the illuminated plate twice, each exposure being for the length of time required respectively for one open sector to pass in front of the plate. The eye thus received in alternation the two stimuli, and the presentation was continued until a satisfactory judgment could be made. Two criticisms, perhaps, may be passed on this method of presenting the stimuli. (a) By giving the stimuli in succession rather than simultaneously a time error was involved which could be reduced, of course, by increasing the rapidity with which the stimuli were presented; but (b) if the stimuli were given in rapid succession it would scarcely be probable that each excitation would be uninfluenced by the preceding excitation. There are two ways in which this influence might be exerted. (1) The after-effect, positive or negative, depending upon the interval between the successive exposures, might modify the effect of the succeeding exposures; and (2) the lag in the response might itself be modified by a repetition of exposures to the same stimuli either through the carrying over of the excitation from impression to impression or by increasing the excitability of the light-sensitive structures. MacDougall's method of determining the point of maximum sensation is as follows. One of the open sectors was made of such a breadth that, with the speed of rotation used, the time of exposure of the eye to the illuminated plate was less than was required to arouse the maximum response. The width of the other sector was then made enough greater than the first to produce a just noticeable difference in sensation. Next the width of the second sector was made the standard and the width of the first was increased until a just noticeable change was produced. This alternative use of the two sectors as standard and comparison, was continued until the maximum was reached. So long as the time of exposure to each was less than is required to arouse the maximum response in sensation, the comparison sensation was judged more intense than the standard. When this time had been reached for the standard and exceeded by the comparison, the comparison was judged darker than the standard. The method thus affords the possibility of detecting the time required to arouse the maximum response in sensation within the limits of change required to produce a just noticeable difference in sensation. MacDougall's method of grouping the judgments for the purpose of detecting the maximum is obviously much superior to his method of presenting the stimuli for the individual judgments.

From the results of his experiments MacDougall concludes that the time required for white light to arouse its maximum response varies directly with the intensity of the light used; and that colored lights of equal photometric value all require the same time to produce their maximum effect in sensation.

C. *Recapitulation.* It has long been recognized that the visual sensation does not come at once to its maximum value. The fact was noted by Bacon in 1711, by Beudant in 1815, by Plateau in 1834, and by Swan in 1849; but it was not until the experiments of Exner in 1868, that the work of determining the development-time of visual sensation was definitely begun. Various methods have been used by different investigators and different results have been obtained. As stated in the introductory chapter, this is one of the reasons that has induced us to take up the problem anew. In general two classes of methods have been used, the direct and the indirect. Under the first class may be included the methods used by Swan, Charpentier, Lough, Broca and Sulzer, Martius, Büchner, and Berliner; and under the second, the methods of Exner, Lamansky, Kunkel, Dürr, and MacDougall. No two of these methods have been the same in all essential details and little, if any, systematic attention has been given by the individual investigators to a comparative study of methods. Moreover, the control and specification of conditions has not been such that a comparison can be made from their published works. Without further experimental work, then, a decision is impossible between the conflicting results that have been obtained. The chief points of interest in the earlier investigations have been as follows,—(1) A comparison of the times required for the different color sensations to reach their maximum; (2) a comparison of this time with the time required for colorless sensation; and (3) a determination of the effect of the intensity of the light on the time required for a sensation to reach its maximum. With regard to these points there has been the widest disagreement, especially with regard to the first. Kunkel, Lamansky, Broca and Sulzer, and Martius, for example, found that the different sensations of color require different lengths of time to reach their maximum; while MacDougall, Dürr, and Berliner concluded that the development time is independent of the color value of the sensation. Furthermore, with regard to this point a great deal of disagreement in detail is found even among those who have agreed on the more general aspect of the question. That is, while Dürr, Berliner, and MacDougall agree that all the colors come to their maximum in the same length of time, they disagree with regard to the value which should be assigned to that time; and while Kunkel, Lamansky, Broca and Sulzer, and Martius agree that the colors all require different lengths of time to reach their maximum, they disagree both with regard to the length of time which is required and to the order in which the colors should be placed with regard to the time that is required. Only the last of these points of disagreement, the order of rapidity with which the colors came to their maximum, will be considered here. For a detailed comparison of the individual value of the time required the reader is referred to Table VI. For Kunkel red produced its maximum effect in sensation the most quickly, blue next, and green the least quickly; for Lamansky red was the slowest of the three to come to its maximum; and for Broca and Sulzer the order from fastest to slowest was blue, green, and red. In the work of Broca and Sulzer alone, the colors were equated in brightness. For the others with the exception of Martius, not even a rating of the colors in order of brightness can be given. Martius' colors' from lightest to darkest were yellow, green, blue and red.

The four men who have worked both with white and colored lights have agreed that there is a characteristic difference in the time required for the two kinds of light to produce their maximum effect in sensation. Again, however, there is quite a wide disagreement with regard to what value should be assigned to this difference. Dürr found, for example, that colored light requires approximately twice as long to produce its maximum effect in sensation as white light; for MacDougall and Martius the time which is required for a colored light to produce its maximum effect lies within the range of values obtained for the different intensities of white light used, and for Broca and Sulzer, the time required for white light to arouse its maximum response is longer than for blue, and shorter than for red and green. A fair comparison, however, cannot be made from the results obtained by Martius, Dürr, and MacDougall because they have made no attempt to specify the intensities of their colored lights.

Concerning the last point mentioned above there is more agreement than for the other two. Dürr alone of those who have made the determinations of the time required for the sensation to reach its maximum, has found that the time is independent of the intensity of light used. For white light his results are contradicted by those of Exner, Broca and Sulzer, Martius, MacDougall, Charpentier, Lough, and Berliner; and for colored light by those of Kunkel, Broca and Sulzer, and MacDougall. Swan alone agrees with him, and Swan, it will be remembered, came to his conclusions only from some rough experiments with reference to the effect of short exposures on the eye's response. The experimental determination of the time required for the sensation to reach its maximum was not made by him nor was systematic work done for times of exposure shorter than this.

In order to bring all the foregoing facts into a briefer compass for the sake of a more ready comparison Table VI has been compiled. This table shows the range of work covered by each investigator and within what limits the results obtained vary.

TABLE VI

Showing the results obtained by previous investigators for the time required for sensation to reach its maximum value.

	Sensation	*Stimulus*	*Wave-length specifications*	*Intensity specifications*	*Time required for sensation to reach its maximum value (seconds)*
Exner 1868	White	White paper		Unit is light reflected from white paper illuminated by oil lamp	
				1 unit	0.150
				1/2 "	0.200
				1/4 "	0.246
				1/8 "	0.287
Kunkel 1874	Colors	Spectrum	None	Prismatic spectrum of a petroleum lamp	
	Red			No reduction in intensity	0.057
	Green				0.097
	Blue				0.101
	Green			Intensity decreased one-half	0.133
	Blue			Intensity doubled	0.091
Charpentier 1887	White	White paper illuminated by skylight		Five intensities in arbitrary scale used	0.014-0.049
Lough 1896	White	White paper illuminated by a 1 cp. lamp			
				0.097 meter-candles	0.148
				0.388 "	0.110
				1.552 "	0.100
				6.208 "	0.085
				24.832 "	0.090
Dürr 1902	White	Milk glass illuminated by carbon lamp filters		Two intensities Values not given	0.266
	Colors				
	Red		700-610μμ	Two intensities	
	Yellow		Not given	"	
	Green		510-486μμ	"	0.541
	Blue		476-440μμ	"	

TABLE VI (continued)

	Sensation	*Stimulus*	*Wave-length specifications*	*Intensity specifications*	*Time required for sensation to reach its maximum value (seconds)*	
Martius 1902	White	White paper illuminated by an arc lamp		Ratios in an arbitrary scale		
				1	0.013	
				1/2	0.039	
				1/4	0.053	
				1/8	0.066	
				1/16	0.078	
				1/32	0.093	
	Colors	Colored glass and gelatines	None	Order of brightness of colors: Yellow, Green, Blue, Red		
	Yellow				0.020	
	Blue				0.036	
	Green				0.048	
	Red				0.090	
					Obs. I	Obs. II
Broca and Sulzer 1903	White	White paper illuminated by an arc lamp		170 meter-candles	0.040	0.030
				126 " "	0.065	0.035
				885 " "	0.065	0.050
				64.5 " "	0.125	0.050
				42.5 " "	0.125	0.070
				32.4 " "	0.150	0.070
				21.0 " "	0.150	0.125
				16.2 " "	0.200	0.125
	Colors	Filters	None	See figure III	See figure III	

Table VI (continued)

	Sensation	*Stimulus*	*Wave-length specifications*	*Intensity specifications*	*Time required for sensation to reach its maximum value (seconds)*
McDougall 1904	White	Milk glass illuminated by lamp		Ratios in arbitrary units	
				1	0.049
				1/2	0.055
				1/4	0.061
				1/8	0.066
				1/16	0.078
				1/32	0.089
				1/64	0.100
				1/128	0.127
				1/256	0.142
				1/512	0.150
				1/1024	0.183
				1/2048	0.200
	Colors	Gelatines	None	Equal in brightness	Same for all colors
				Three intensities	
				Value not given	0.100-0.142
Büchner 1906	White	Milk glass illuminated by lamp		0.027 meter-candles	0.230
				0.145 "	0.200
				0.260 "	0.120
Berliner 1907	Colors	Papers and gelatines	None	None	Same for all colors 0.300

III. EXPERIMENTAL

A. *Purpose of Investigation*

As stated in our introductory chapter the plan of this investigation includes the following points. (1) An experimental examination of the older types of method will be conducted, to find out which have sufficient feasibility and precision, to warrant their further use in connection with our work on the problem. (2) A comparative determination of the relative lag of colored and colorless sensation with lights of the same photometric value, for the same observers, under the same experimental conditions will be made by the methods selected. And (3) the determination will be made by three new methods. By this plan it is hoped that the following objects will be accomplished. (a) Not only will more accurate knowledge be gained of the time required for the visual sensations to come to their maximum, but a comparison of values will be made during the course of their rise. (b) Data will be obtained which are directly applicable to photometric methods in the interest of which primarily the study was undertaken. And (c) through the intercomparison of methods with the same observer and under the same experimental conditions a selection will be made of those which combine the following features: logical sureness, precision, and convenience and feasibility of application.[30]

---

[30] For example, in studying the conditions which decrease the power of the eye to sustain clear seeing under different lighting conditions two factors should be taken into account,—the depression of retinal functioning, and the depression of muscular functioning. (For a description of tests for depression of muscular functioning, see C. E. Ferree: "Transactions of the Illuminating Engineering Society," 1913, VIII, pp. 40-60. Also C. E. Ferree and G. Rand, ibid, 1915, X, pp. 410-413 and 454-491; 1916, XI, pp. 1122-1130.)

A depression of retinal functioning may show itself in any or in all of the following ways: (1) A lessening in sensitivity to colored or colorless lights as tested by the momentary responses; (2) an increased susceptibility to exhaustion or fatigue; (3) a decreased rate of recovery from fatigue; and (4) an increased inertia or lag in giving the maximum response.

B. *New Methods of Making the Determination*

The new methods which are being presented here for the first time, are designed not only to ascertain the time in which a given stimulus produces its maximum effect in sensation, but also to make possible a comparison of the effects produced at different times before the maximum effect is attained. There are two possibilities of making accurate and reproducible judgments of sensation, namely, the judgments of equality and of just noticeable difference. Upon these judgments must all quantitative work in psychology ultimately rest. Both of them have been utilized in the development of the methods mentioned above. The development of these methods has in fact been just the systematic application of these two judgments to the problem in question, in accord with the purposes mentioned at the beginning of the paragraph.

(1) *Method 1.*[31] This is a direct method and from its similarity to the photometric method of the same name it may be called a direct comparison, or equality of brightness method. The principal feature of this method is to establish a series of standard sensations graded in brightness to which the sensation whose temporal course is under investigation may be matched at various points in its rise to the maximum. In a previous chapter we have pointed out that Broca and Sulzer, Büchner, Lough, Berliner, Charpentier, and Martius failed to provide such a series of standard sensations graded in brightness and have indicated in each case wherein the deficiency lay. Obviously such a standard may be obtained by establishing a reproducible series of just noticeable differences in sensation for the same wave-lengths of light that arouse the sensation whose development is to be

Tests covering the first three of the above points are already in use in this laboratory. It would add materially to the completeness of our equipment for this work, if a method which is sufficiently brief and convenient in application to suit the purposes of a laboratory test, could be found for the fourth point.

Up to this time, we regret to say, our use of the older methods has been attended with somewhat discouraging results.

[31] For the suggestion of Method 1 and Method 2 the author is indebted to Dr. Ferree.

investigated. This series should begin at zero and rise to a value as high at least as the maximum for the sensation under investigation.

Before going further into the details of this new application of the equality of brightness judgment to the work of determining lag a word of general perspective may not be out of place even at the risk of involving repetition. As we have pointed out in the preceding chapter, the difference in the problems to which this judgment is applied when it is based on making a photometric comparison of two light intensities and when it is used in detecting the lag in sensation, should be kept clearly in mind. In the former case the eye is exposed to the standard and comparison lights for equal intervals of time and therefore no account need be taken of the changing effect on sensation of different lengths of exposure. In the latter case the eye must be exposed to the standard and comparison lights for different intervals of time, and the changing effect on sensation of different lengths of exposure is of great importance. In the former case, too, sensations are matched with the object of equalizing light intensities and of giving a relative rating of the two light sources; in the latter, the problem ends with the matching of the comparison to the standard sensation. In some respects the problem in the latter case is essentially simpler than in the former for a rating of light sources in terms of their power to arouse equal sensation values at the photometer screen is not required. It is more complicated, however, in that, if the brightness of sensations aroused by different lengths of exposure to the comparison light are to be compared, the sensation aroused by the standard light must have a definite place in a graded series. That is, if the sensations aroused by the comparison light are to be graded from minimum to maximum, the method must provide a series of standard sensations graded in brightness to which the comparison sensation for the different lengths of exposure may be matched. In the use of this method by previous investigators, as we have already noted, such a series of graded standard sensations has not been provided. In their work the rating of the comparison sensation was in terms of a series of standard stimuli and a proportional rating was assumed for the sensations aroused by these stimuli, in spite of the fact that in some of the cases the eye was exposed for different lengths of time to the standard stimulus and in all cases no account was taken of the comparative rates of exhaustion of the eye to light of different intensities and of different wave-lengths. It is in fact for the purpose of establishing directly a series of standard sensations, graded in brightness and obtained with a standard time of exposure, that we have taken up the method anew. In establishing this series the rating will be made directly in terms of sensation. It will not be assumed from intensity relations existing between the stimuli as has been done in previous work, in which in one case at least the decay of sensation was disregarded altogether (Broca and Sulzer), and in all of the others no account was taken of the variability in the rate of decay with lights of different intensities. Obviously the just noticeable difference series, previously mentioned, should furnish the standard needed. This series should be for the same wave-lengths of light which are used to arouse the comparison sensation, and should begin at zero and rise to a value as high at least as the maximum of the sensation whose development is under investigation. How such a series may be established, will be understood better in detail when we come to a description of the apparatus. It will be sufficient to say at this point only the following. Two contiguous comparison surfaces are provided, either of which may be illuminated by the standard light in slightly varying amounts. In the beginning, for example, surface 1 is dark and surface 2 is illuminated until it is just noticeably lighter than surface 1. Surface 2 is then made the standard and the light is increased on surface 1, until it is just noticeably lighter than surface 2; and so on, until the desired number of just noticeably different brightnesses are obtained. In each case a record must be kept of the precise amount of light used for each member of the series, and of the lengh of time of exposure which should preferably be kept constant. In the following experiments the first of these requirements was attained by means of a double micrometer slit, adjustable to thousandths of a millimeter, which was placed in the end of the collimating tube of the spectroscope. A very important feature of the method is, of course, some means of guaranteeing the reproducibility of the series of standard sensations.[32] This we believe can be done for a practised observer within the limits of certainty that are accepted in making many physical determinations. For example, the sensitivity of a delicate galvanometer (of the Paschen type, for example), may vary from day to day and for this reason a check is made on its sensitivity prior to the measurement of an unknown current. That is, the instrument is provided with a sensitivity tester and tests are made as the need of the work requires. This can be done also for the eye. That is, like the galvanometer, if the sensitivity of the eye varies from day to day, its scale of responses will vary correspondingly, but just as a check may be had on the sensitivity of the galvanometer, so may it be had on the eye.[33] This may be accomplished in the following way. As a preparation for the work the just noticeable difference series should be determined until a high degree of reproducibility is obtained from series to series. When a satisfactory degree of practice is reached it will probably be sufficient prior to a given experiment to conduct a brief test to see that the sensitivity of the eye of the observer has not changed. This would probably consist of a test of the scale at only one or two points. One is usually considered sufficient in the case of a galvanometer, for example, but, if this is not sufficient, it does not take long for a practised observer to run over the whole scale. This has the advantage that, if the sensitivity of the eye has changed, a new scale of sensitivity is established which can be used for that particular time and experiment. With a practised observer and the proper control of conditions we have found so little variation in the series from time to time that the check provided by the briefer test has been amply sufficient.

---

[32] The accumulated error through a series with a practised observer has never amounted to a just noticeable difference. For example, an intensity of stimulus which has at times given eight just noticeable differences of sensation has never at another time given seven or nine just noticeable differences. That is, the points in the sensation and stimulus series may vary from sitting to sitting but never as much as is represented by a just noticeable difference in sensation.

[33] It should not be understood that we wish to carry this analogy too far. If, for example, the sensitivity of the galvanometer changes from day to day, a corrective factor can be determined and applied to convert the variable deflections into some value taken as a standard. The correction of the eyes to a standard sensitivity can not, of course, be done, as yet. All that can be done, is to refrain from working when the test shows a significant change in the eye's sensitivity.

Once the standard series of sensation is established the points in the rise of the comparison sensation to its maximum may be determined in either of the following ways. (1) The standard series may be reproduced, point by point, and the exposure time of the comparison light be adjusted until the sensation aroused just matches the standard sensation; or (2) the inverse of this procedure may be used. That is, different exposure times may be selected for the comparison light and a member be selected from the standard series which matches it. In either way the resulting values of the sensations aroused by different intervals of exposure of the comparison light may be rated within a limit of accuracy which falls at any point within a just noticeable difference of sensation. The method permits then of placing in a just noticeable difference series the sensation aroused by exposing the eye to a given intensity and composition of light for different intervals of time. It also permits us to compare in case of lights of different composition the intervals of exposure required to give us the third, fourth, etc., just noticeable difference in the brightness scale; and, if we are allowed to assume that brightness values which are three, etc., just noticeable differences removed from zero are equal, we should be able to detrmine what lengths of exposure for the different wave-lengths of light are required to produce equal sensation values. Repeated trials have lead us to believe that this assumption is in most cases justifiable. That is, repeated trials have shown that a sensation of any color, built up by getting three just noticeable differences from zero matches in brightness the sensation of another color, also built up by getting the third just noticeable difference from its zero.

(2) *Method 2.* This may be classed as an indirect method and rests on the fundamental assumption that the more intense a sensation is, the greater will be the number of just noticeable

differences between it and the zero sensation. That is, considering the principle in relation to the determination of the time required for a sensation to reach its maximum brightness, we would assume that the time of exposure which would give the greatest number of just noticeable changes in brightness for a stimulus of a given value, is the interval which would cause that light to arouse its maximum brightness response in sensation. In this method, a series of just noticeable differences is made for the appropriate range of time of exposure. In each series of measurements the time of exposure is kept constant; and the stimulus is varied from the zero value to the intensity of light under investigation and the number of just noticeable changes in brightness is determined. The time of exposure which gives the greatest number of just noticeable changes in brightness for a given intensity of light, is considered to be the time most favorable for light of that intensity; in other words, the time which just allows the sensation to reach its maximum brightness. Besides determining for us the time of exposure which gives us the maximum brightness the method also provides a means of rating the sensation values for different times of exposure in terms of just noticeable differences from the zero value. It allows, therefore, an opportunity for an inter-comparison among the colors of the effect of time of exposure on the brightness value of the sensation. The comparison is made in terms of just noticeable differences from the zero values. For example, from the results we may determine what lengths of exposure give sensations which represent an equal number of just noticeable brightness changes from the zero value; or what is perhaps less sure logically, the relative rating of the different color-sensations for equal times of exposure in terms of just noticeable changes from the zero value of sensation.

(3) *Method 3.*[34] With a logical sureness and accuracy equal to any of the longer methods described above, this method combines much greater ease and convenience of application. The length of time required to make the determinations and the ease with which they may be made, renders it in fact feasible as a

---

[34] For the suggestion of this method the author is indebted to Dr. Rand.

practical laboratory test for changes in retinal functioning (See this paper, p. 44). As was the case in Method 2, the judgments made are of just noticeable changes in sensation; but the changes are produced by varying the time of exposure, instead of the intensity of the stimulus. Consequently, just as the former might be called an intensity just noticeable difference method, so might this be called a time just noticeable difference method. That is, in this method, the just noticeable changes in terms of which the sensation is graded in its rise to the maximum, are produced directly by varying the time of exposure. The series is started with a zero value of time of exposure to the light in question, as standard; and the time of exposure of the comparison light is varied, until a just noticeable sensation is produced. This is then made the standard for the next observation, and the time of exposure of the first light is varied until the sensation produced is just noticeably brighter than the standard. So long as the comparison is brighter than the standard, it is obvious that the maximum response in sensation has not been reached. When it has been reached, the standard and comparison are judged equal; and, when it has been exceeded, the comparison is judged darker than the standard. From the results obtained by the series the following points may be determined,—(a) the time required for the stimulus light to arouse its maximum effect within the limits of a just noticeable difference; (b) the relative values of the sensation in a just noticeable difference series, as it rises to its maximum; and (c) a comparison of the intervals of exposure required for lights of different color to arouse sensations representing an equal number of just noticeable changes in brightness from the zero of sensation. If only the time required to give the maximum response is wanted, the series may be abbreviated. That is, it may not be started at the zero value; but at some point above this, safely below the point of maximum sensation. This abbreviation would lessen a great deal the labor involved in making the test and would give as a result probably all that need be known to make the test meet the practical purposes mentioned at the beginning of this section. So modified, this test is the shortest and most feasible of any that we have as yet been able to devise.

C. *Apparatus*

It is obvious that in order to use all of the methods described above, we must be able to produce two stimuli, independently variable in intensity, identical in composition, and for color work as nearly homogeneous in wave-length as is possible. We must also be able to present these stimuli to contiguous parts of the retina and to vary their periods of exposure independently by known amounts and within wide limits. The description of the apparatus which was adopted as best suited for the carrying out of these methods naturally divides itself into two parts,—(1) the apparatus for obtaining the two stimuli and varying their intensity; and (2) the apparatus for presenting the stimuli to the eye and for controlling the time of exposure.

The apparatus used for obtaining the two monochromatic lights and for varying their intensity independently and by known amounts has been used for several years in this laboratory for work on the color sensitivity of the retina. The apparatus for presenting these lights to the eye and controlling the time of exposure was designed especially for this work.

In order that the detailed description which will follow may be better understood, it has been thought advisable, first, to trace schematically the course of the ray of light from the Nernst filament, which serves as a source, to the eye of the observer. This has been done in Figure XII.

The light from the Nernst filament (N) passes through the double slit (S_1) and is rendered parallel by the collimating lens (L_1). It then passes through the carbon bisulphide prism (P), and the lens (L_2) focuses the resulting spectrum in the plane of the analysing slit (S_2). The monochromatic light emerging from this slit is focused by lens (L_3) in the plane of the disc (D). Spreading from the plane of this disc, the light is again focused by the lens (L_4) forming an image of the analysing slit (S_2) on a mat white surface at the back of the box (B). This image is observed through a tube (T_3) at the opposite end of the box.

(1) *Apparatus for Obtaining the Stimuli and Varying their*

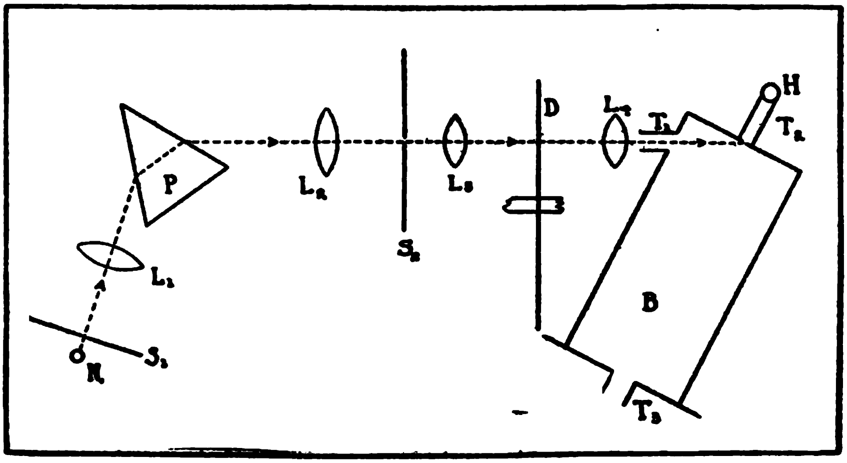


FIGURE XII

Intensity. The apparatus for obtaining the two stimuli and varying their intensities has already been described.[35] A part of this description may be quoted here:

(a) "*The Source of Light.*—The requirements of a source for our purpose are (a) that it should give a light in which all the wave-lengths are represented in sufficient strength both for the needs of the investigation of color sensitivity and for radiometric standardization; and (b) that the light emitted shall be as constant as possible in intensity and spectro-radiometric composition. After trying many light sources we have finally adopted as best for our purpose the Nernst filament. This filament, after having been properly seasoned, gives a light which changes comparatively little through long intervals of time. It has the advantage, moreover, that its shape well adapts it for use with the slit of the spectroscope,[36] *i.e.*, the shape is such as to make it possible to utilize for the illumination of the face of the prism a relatively large proportion of the light emitted. Also by increasing the length of the filament and collimator slit, it is possible to increase in direct ratio the amount of light falling on the face of the prism. When in use, in order that no light shall

[35] C. E. Ferree and G. Rand: "A Spectroscopic Apparatus for the Investigation of the Color Sensitivity of the Retina, Central and Peripheral," Journal of Experimental Psychology, 1916, I, pp. 247-283.

[36] The filament is supported by two stiff wires extending out from the lamp socket and when in use is as nearly as possible in the plane of the slit.

be lost in passing through one or more condensing lenses, the filament is mounted as closely as possible to the jaws of the slit. The filament is mounted in a lamp socket fastened to an upright. In order that the height of the filament shall be adjustable this upright consists of a short rod sliding in a tube fitted with collar and set screw which permits of a movement of the socket up and down, also to right and left. The upright is fastened to the horizontal support, extending out from the collimator arm of the spectroscope by a clamp and set screw which permits of the motion of the socket to right and left and to and from the collimator slit. The Nernst filament used by us is designed to be operated at 110 volts on a D. C. current, and has a carrying capacity rated at 1.2 amperes. When operated at its maximum capacity on a direct current, we have found, however, that the life of the filament is short. Satisfactory results have been obtained by us only when it is operated at or below 0.6 amperes. In series with the ballast, therefore, which is ordinarily used for the reduction of the current from the line and to compensate for the change in the resistance of the Nernst material as its temperature varies, we have found it necessary to use additional resistance. This resistance is needed not only to cut down the current to the desired value, but to correct for fluctuations in the line. Two coils are employed, one coarse and one fine. The former is used to cut down the current to approximately the desired value, and the latter to correct for the fluctuations in the line. Both are in the form of adjustable rheostats. The second is of special construction to give the small changes needed. It consists of a cylindrical coil of wire wound on an insulated core of brass tubing and is operated by a screw motion in such a way as to give the effect of a slider on a single wire. This rheostat is described in greater detail on p. 275. It is not on the market but can readily be constructed to order by a laboratory mechanician. The finely graduated control afforded by it not only makes it possible to correct for fluctuations in the line, as is stated above, but also to compensate for the slow decrease in the carrying capacity of the Nernst material with use. The current consumed is measured by a Weston ammeter graduated to 0.02 amperes. Operated with this type of control the light flux obtained from the Nernst may be kept constant within the limit of change that can be detected either by the radiometer or the eye.

(b) *"The Spectroscope.*—In addition to the usual features attaching to a good spectroscope, our instrument was designed especially to meet the following needs. (1) To answer all the purposes for which a source of colored light may be used in the investigation of color sensitivity, a wide range of intensity is needed. In order too that an adequate radiometric standardization be made, it is especially desirable that light of high intensity be available. (2) If the spectroscope is to be used in conjunction with a campimeter, it is necessary that the objective arm remain in a fixed relation to the stimulus-opening of the campimeter screen and that some convenient and accurate means be had of changing the wave-lengths without changing the position of the objective. The first of the needs was met by employing a colimator slit long enough to admit the amount of light needed, and a prism and lenses large enough to take care properly of the amount of light admitted. The second point has been met by us in the two following ways, one of which is technically more correct perhaps than the other. (a) The spectroscope was mounted on a stationary base supported by levelling screws. In fixed relation to this base a slit was permanently mounted to separate out the wave-lengths which are to fall on the stimulus-opening in the campimeter screen. In order to be able to change the wave-length of the light falling on the slit the stationary base carries a track along which the whole spectroscope may be shifted by very small amounts. This movement is made by means of a screw of fine thread turned by a wheel 4½ inches in diameter. This track and the base of the spectroscope carry a Vernier scale graduated to 1/50 of a mm. by means of which any previous setting may be accurately reproduced, and in terms of the readings of which the spectroscope may be calibrated in wave-length. This method of changing wave-length not only provides abundantly for small changes in wave-length but it obviates any necessity for readjustment of the collimator arm which would be the case were the wave-lengths changed, for example, by rotating the prism. In case the wave-length is changed by this device, the prism is set for minimum deviation for the D-line, and this adjustment is kept throughout. An adjustment which gives minimum deviation for the D-line alone is generally considered by spectroscopists to be adequate for the work in the visible spectrum when the light is passed through only one prism. Kayser, for example, says:[37] 'Bei den Apparaten mit nur einem Prisma verzichtet man für gewöhnlich darauf, alle Wellenlängen unter dem Minimum zu beobachten, sondern stellt das Prisma fest auf, so dass etwa die D-Linien unter dem Minimum durchgehen. Die Dispersion ist bei einem Prisma so gering, dass dies gewöhnlich genügt. Das ist aber nicht mehr der Fall, wenn man über die Grenzen das sichtbaren Spectrums hinausgeht, und so hat vielleicht suerst Langley bei seiner Untersuchung des ultrarothen Theiles des Sonnenspectrums an einem Apparat mit einem prisma eine Vorrichtung beschrieben, welche automatisch das Minimum erhält, und welche wegen ihrer Einfachheit seitdem oft verwandt wird.' (b) In order that minimum deviation may be had automatically for all wave-lengths falling on our analyzing slit, one of our spectroscopes was built with a minimum deviation attachment. A schematic representation of this attachment and the prism in position for use is shown in Fig. 4. In this figure, P represents the prism so placed on the prism table that its refracting angle is bisected by one of the radii of the table; PB and PA represent respectively the collimator and objective arms which are fastened to the stem of the spectroscope independent of the prism table; TD represents an arm fastened to the prism table in suich a position as to be continuous with the radius of the table which bisects the refracting angle of the prism; AE and BE represent two rods of equal length which connect PA and PB at points equidistant from the center of the table to a collar which is free to play back and forth along the arm TD. M is a micrometer screw with a graduated cylindrical head, which is used to move the collimator arm through the small angles needed to give the change of wave-length. Opposed to this screw is a plunger *p* working against the spring in the case C. When the screw advances it moves the collimator arm for-

---

[37] Handbuch der Spectroscope, I, p. 510.

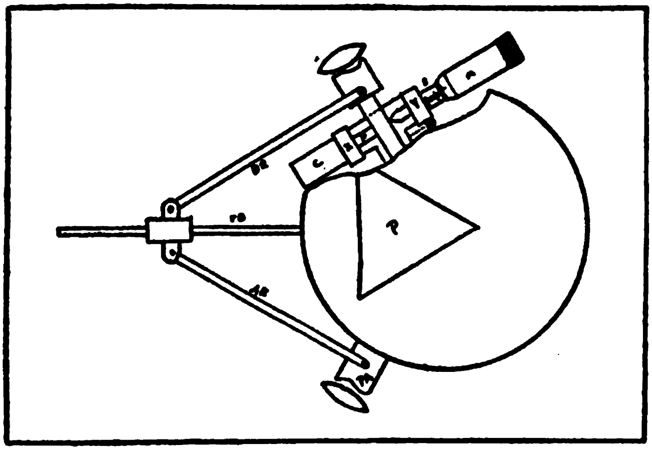


FIGURE 4

ward and when it recedes the collimator arm is made to follow it by the push of the plunger *p*. The screw and plunger are supported by a curved arm coming off from the stem of the spectroscope, which can be clamped rigidly in any position which may be desired. This arm ends in two right-angled extensions, one of which carries the screw and the other the plunger. Between X and Y, the vertical arms of these extensions, the collimator arm is moved to give the change of wave-length. The micrometer screw passes through a fixed sleeve S graduated in fortieths of an inch. As the screw advances the cylindrical head telescopes on the sleeve S, one of the graduated spaces being traversed with each complete turn of the screw. The forward end of the cylindrical head is bevelled and on the circumference of the bevelled edge is a scale of equal divisions graduated in twenty-fifths of an inch. By means of this scale and the scale on the sleeve S, the advancement of the screw can be read in thousandths of an inch."

(c) "*Collimator Slits.*—One of these slits is of the usual type having carefully beveled knife-edged jaws 12 mm. long, with a micrometer adjustment of width graduated to thousandths of a mm. Such a slit may be used (a) to make a reduction of the spectrum as a whole or of any part, and the reduction may be computed directly from the slit width provided the source is uniformly luminous over the surface exposed; or (b) one part of the spectrum may be made in turn to sustain, within limits and under the conditions mentioned above, any ratio that may be desired to any other part of the spectrum, providing the original intensities from which the reductions are made, are known. This slit, however, is not adapted to just noticeable difference determinations for a given color or range of wave-lengths. The second slit is especially designed for just noticeable difference work. This slit is constructed so that its upper and lower halves are independently variable in width. It was designed especially for the new methods we are using for a quantitative comparative determination of the retina's inertia to the different wave-lengths of light in which just noticeable differences are employed. In the short exposures used in tracing the sensation from the threshold to its maximum, it is obvious that the sectored disc could not be employed in making the variations needed for just noticeable differences. While designed to meet this special need we have found it to be a very convenient means of making the variations needed for much of the general work in just noticeable difference determinations. The slit is formed by three knife-edged jaws. That is, one jaw of the slit is made in one piece and is 12 mm. long; the other jaw is made in two pieces, each 6 mm. long. One edge, the upper for the upper jaw and the lower for the lower jaw, is beveled to fit into a dovetailed guide cut in the frame. The other edge of each jaw is held in place by a slender close fitting key of appropriate length. The jaw, 12 mm. long, is stationary and the other two jaws are made to move away from it by two independent micrometer screws operated by drum heads graduated to thousandths of a millimeter. In operation the source is adjusted so that one edge of its equally luminous surface is flush with the stationary jaw and the other jaws are pulled away from it exposing as desired different widths of this surface. In a just noticeable difference series one half of the slit is held constant and the other is varied to give the just noticeable difference. When the width of slit needed for this is obtained, that half of the slit is held constant and the other is varied, and so on until the series is completed."

(2) *Apparatus for Presenting the Stimuli to the Eye and for Controlling the time of Exposure.*

(a) *The Lens System.* The apparatus for presenting the stimuli to the eye and controlling the time of exposure consists of the lenses L_3 and L_4, the exposure apparatus and the stimulus-box. The lens L_3, which as stated above focuses the light emerging from the analysing slit S_2 in the plane of the exposure discs, has a focal length of 8.5 centimeters. Two things are required of this lens—(a) it must focus the light accurately on the disc, and (b) the bounding line between the upper and lower halves of the image it forms of the slit S_2 must coincide with the bounding line between the two sets of exposure discs.[38] That is, in order that the time of exposure of the light coming from each half of the double slit shall be independently variable, the open sector of one set of the exposure discs must receive all of the light from one half of the slit; and the open sector of the other set of discs, all of the light from the other half. Both of these points are conveniently accomplished by having the holder of the lens mounted in an adjustment which gives to it two motions,—one back and forth along the path of the light to adjust the focus, and the other in the vertical to provide for the proper placing of the image on the disc. The lens L_4, which focuses the image of the double slit in the stimulus box, has a focal length of 5 centimeters. It is mounted in a holder the base of which moves in a grooved track parallel to the path of the stimulus light. To provide for accurate focusing, this base is operated by means of a finely threaded screw.

---

[38] This coincidence is, of course, not exact, since the bounding line between the two slits is straight while that between the two sets of discs is circular. In reality the boundary between the two images is tangent to that between the two sets of discs. However, since the width of the image of the slit is only 1 millimeter and the radius of the inner set of discs is 14 centimeters, the deviation of the two boundaries from exact coincidence is negligible for the purposes of these experiments.

(b) *The Exposure Apparatus.* The exposure apparatus is made up of the rotation apparatus and two specially constructed sectored discs. The rotation apparatus consists of a heavy V-shaped stem with carefully turned bearings, in which rotates a spindle fitted with a special arbor and chuck to hold the exposure disc. Since only a single exposure is wanted for any single observation, the discs are driven by means of a gravity or fall device. That is, attached at its middle to the other end of the spindle, is a rod one meter long which carries on its two arms equal weights. These weights are lead cylinders, which slip on to the rod and are held in position by means of set screws. The rod is graduated in millimeters, so that at any time the position of the weight may be read directly from the scale. Before an exposure the rod is held in position by a catch, the height of which is adjustable. When the catch is released the weight falls rotating the disc D with a velocity depending upon the height of the catch and the position of the two weights on the rod. After the fall, the other end of the rod is caught automatically by another catch. To re-adjust the device for a new observation, the rod is brought back to its original position by hand.

Another factor in determining the length of time of exposure, is the displacement of the open sector from the path of the stimulus light, or its position in the circle of rotation; for, obviously, upon this will depend, with a given height of the catch and a given position of the weights on the driving rod, the amount of acceleration which is given to the disc before the open sector reaches the path of the stimulus light. In making the exposure, the disc was always so adjusted on the axis of rotation apparatus that the light was shut off from the stimulus box when the driving rod was held by the first catch; then, when released one complete exposure had been made before it was caught by the second catch. Since, as stated above, the position of the open sector in the circle of rotation is an important factor in determining the length of exposure which will be obtained with a given value of open sector, height of the catch and position of weights on the driving rod, it is very important to have this position the same at the beginning of each exposure. This was accomplished by drawing a radial line on the disc, when the open sector was in the position desired, so that it just coincided with one edge of the band of stimulus light which was focused on the disc by the lens L_3. A fixed zero is thus established from which the movement of the open sector may be begun at each successive exposure.

(c) *The Stimulus Box.* In careful determination of lag, it is necessary that the stimulus be presented to the eye without the admixture of stray light. In order to accomplish this, the stimulus box B was devised. This apparatus which is very simple in construction is made up of the box proper, a screen to receive the image formed by the lens L_4, and three tubular openings. The box is made of wood and is 52.7 centimeters long, 10.5 centimeters wide, and 11.5 centimeters high. It is painted a mat black, inside and out, and care has been taken to make it light proof except for the three openings mentioned above. One of these openings T_1 is for the admission of the stimulus light; one T_2 for the admission of the light which illuminates the fixation point; and one T_3 for the passage of the stimulus light to the observer's eye. Further to guard against the admission of stray light into the box, these tubes are from two to six inches in length and are painted a mat black on the inside. The screen to receive the image formed by the lens L_4 is at the back of the box in line with the axis of the tube T_3. This screen is of cardboard and is fitted into a holder which can be turned about the vertical axis by a knob. This provision for rotation is made in order that the screen may be brought, in every case that may arise, into a satisfactory optical relation with the lens L_4 and with the eye. In order to provide a suitable surface on which to view the image formed by the lens L_4, a narrow vertical strip in the center of this screen is covered with magnesium oxide deposited from the burning metal. This surface has a reflection coefficient of 90 per cent and is just large enough to accommodate the image, which is 4 millimeters wide and 15 millimeters high. The remainder of the screen is covered with velvet black paper which has a coefficient of reflection so small as to be negligible in these experiments. In order that the observation shall be begun with the eye accurately adjusted for its stimulus, a fixation point is provided at the exact center of the image formed by the lens L_4. A preliminary adjustment of the eye is especially necessary in work of this kind because of the short interval of time for which the stimuli are exposed. This fixation point is devised in accord with the following requirements. (a) It must be luminous else it can not be seen in the dark field of vision produced by the stimulus box. (b) It should disappear at the instant of the appearance of the stimulus or its light will, mix with the stimulus light and modify its action on the eye, and interfere also with the brightness comparison of the two halves of the image. And (c) it should be small and no brighter than is necessary to be seen clearly in order that it may not act to any considerable degree as a pre-exposure and modify, thereby, the sensitivity of the eye to the stimulus light. The last two of these requirements may seem of small consequence; but when methods are used which depend upon the judgment of just noticeable differences, the amounts of sensation involved are as small as possible and the influence of extraneous factors should be reduced to a minimum. This fixation point was obtained by pricking a small hole through the stimulus card at the exact center of the image formed by lens L_4 and illuminating it from behind by a two candle power incandescent lamp, contained in the small lamp-house F. This lamp-house is connected with the stimulus box by the tube T_2 which prevents the light from scattering into the surrounding room; and contains, besides, absorbing screens of the number and diversity needed for reducing the intensity of the light. In order that the fixation point should disappear simultaneously with the appearance of the stimulus, the following provision is made. The current from the lamp passes from the source to the axis of the rotation apparatus and then to an adjustable collar about this axis, having a sector-like extension. When the rod is held by the catch, this extension makes connection with a flexible brass strip and the current passes through the standard supporting the strip to the lamp and back to the source. The extension is so arranged that, with the rotation of the axis, the connection is broken just as the open sector of the disc allows the light to pass through and form an image on the magnesium oxide surface on the screen at the back of the stimulus box. The breaking of the circuit causes the dot which serves as a fixation point to disappear and prevents its interference with the judgment of the brightness of the lights. A fixation point which shall remain visible until just before the appearance of the stimulus is found to be necessary for accurate work, since otherwise the eye is not in position to receive the stimulus and the shifting of it into position may change the effect of any given exposure.

As has been mentioned before variations in the time of exposure of the stimulus can be introduced in three ways,—(a) by changing the relative position of the weights; (b) by varying the amount of open sector of the disc; and (c) by causing the open sector to cut the beam of light at different points in the revolution of the disc. In order to obtain the exact time of exposure of the stimulus under each of the variations used in the experiments two methods were employed. In the first the procedure was as follows: On the edges of the open sector of the disc were soldered small extensions perpendicular to the disc. These had fine knife edges. Making electrical contact with the knife edges, was the point of a fine coiled spring attached to the end of a horizontal rod. This rod rotated about a vertical standard on a collar, provided with a set screw. By means of the collar and set-screw the rod could be turned until the point of the spring was in the axis of the beam of light which served as the stimulus, and clamped. The spring could then be extended along the axis of the beam until the point just made contact with the knife-edges as they passed. When the contact was made an electric circuit was completed allowing current to flow from the line through a lamp rheostat, the axis of the rotator, the disc, the knife-edges, spring, rod and its supporting standard to an electric magnetic recorder which marked on the smoked surface of the drum of a kymograph. Directly beneath the record of the electro-magnetic marker a time line was traced by a tuning fork vibrating one hundred times per second. By means of this the interval between the contacts of the spring with the two-knife edges, or the length of exposure of the stimulus light, could be determined. In making these determinations the sectors of the disc were set in each position used in the experiments and a time record made. It was found, after a few trials, that unless extreme care was taken when setting the spring to make the touch on the knife edges as light as possible, a lag was occasioned by the friction of the contact. On account of this possibility of error a second method was used to check up the first. In this method a strip of smoked paper was placed over the open sector of the disc, and the position of the two edges marked carefully on the paper. The pointer of the electric tuning fork was then so adjusted to make a tracing on this strip, care being exercised to make contact as light as possible. From the tracing made by this fork when the disc was rotated, the number of vibrations during the passage of the open sector could be noted directly. In both cases the number of whole vibrations were counted and the measurements of all fractions of vibrations were made on a micrometer comparator, which allowed accurate readings to a thousandth of a millimeter. It was found that the time of exposure, as registered by this method, corresponded with that given by the first method, when care was taken to make the contact as light as possible. We have accepted that value as correct, therefore, and have used it in our tables and curves.

D. *Results*

As was stated in the introductory section of this study, one of the purposes of this investigation was to make a re-determination of the time required for sensation to reach its maximum by the more promising of the older methods, under conditions which would make possible an inter-comparison of results. After a preliminary trial of the older methods, the ones selected for further investigation were those of Broca and Sulzer, Exner, Dürr and Kunkel. A brief account of the work with Dürr's method was given in our historical section. No further account will be taken of it here. Another feature of the work was, it will be remembered, to devise new methods and to compare the results obtained by these methods with each other, and with those obtained by the older methods. It will be remembered also that one of the purposes in devising new methods was to make it possible directly to compare the different rates of rise of the sensation towards its maximum value for different wave-lengths and intensities of light, as well as to determine the maximum value itself.

The lights selected for investigation were narrow bands taken from the following regions of the prismatic spectrum of a Nernst filament operated at .6 amperes: 686 μμ 580 μμ, 511 μμ, and 463 μμ; and a mixed approximately white light gotten by re-synthesizing this spectrum by means of a cylindrical lens of suitable focal length. The lights at the different intensities used were made photometrically equal. On account of the excessive amount of time and labor required to make the determinations, especially by the older methods, as great a number of intensities of light and regions of the spectrum could not be investigated as was desired. Only three intensities of light were used, for example, and only four parts of the spectrum. The parts of the spectrum selected, however, were those most important for the problem under investigation; as also were the intensities of light, within the limits that could be attained with our apparatus. The photometric values of these intensities were 0.057, 0.151, and 1.21 meter-candles. The energy values of the four monochromatic lights at these three intensities are given in Table VII.[89] The white light was not radiometered.

---

[89] For the energy meaurements given in this table, the writeer is indebted to Dr. Gertrude Rand.

TABLE VII

		Energy per mm^2 (*watts* $\times 10^{-9}$)	
Wave-lengths	*Photometric value meter-candles*	*At analysing slit*	*At photometer head*
686	1.21	6224.985	65.787
580	"	185.740	1.965
511	"	130.137	1.378
686	0.151	795.422	8.406
580	"	44.337	0.467
511	"	47.657	0.503
463	"	646.040	6.830
686	0.057	238.454	2.522
580	"	2.049	0.221
511	"	1.809	0.191
463	"	189.095	2.001

(1) THE OLDER METHODS

(a) *Broca and Sulzer Method.* In applying the equality of brightness method, heretofore used the most systematically by Broca and Sulzer, the author introduced a modification which, as was stated in the historical part of this study, had not apparently been considered necessary by former investigators, *i.e.* the exposure of the standard light was kept within a very small range of variation. Six seconds was chosen as the best time of exposures to approximate for this light. This choice was determined in part by the following consideration. If the principle of the equality of brightness method, as used by Broca and Sulzer, was to be employed, a longer exposure of the standard light, (2 seconds or more) was demanded than could be obtained with our exposure apparatus. It became necessary, therefore, to adopt a less exact method of regulating and timing the exposure to this light. On this account it was thought better to increase the exposure of the standard light to six seconds, in order that the rate of adaptation of the eye might become sufficiently slow that slight variations in the length of exposure would have little, if any, effect on the apparent brightness of the light.

There are two possible ways of making a series of determinations by this method: either the standard light can be held constant in turn at selected intensities in a graded series and the length of exposure of the comparison light can be varied until the two appear equal; or, conversely, the intervals of exposure of the comparison light can be graded in value and the intensity of the standard light can be varied to give the judgment of equality. The second of these alternatives was chosen for the work here presented.

The method of making the exposure was as follows: The larger of the two sets of discs of the exposure apparatus was adjusted to give a wide open sector; the smaller, to give an open sector of the value required for the comparison light for the case in question. These sectors were so turned about the axis of the rotator that the edges of sectors that were to terminate the exposures to the two lights were in the same radial line, and the standard light passed through the open sector of the larger disc as closely in front of the forward edge of the open sector of the smaller disc as was possible. This adjustment accomplished three things: (a) The exposure to both lights was terminated simultaneously; (b) prior to the rotation of the disc, the comparison light was shut off from the stimulus box; and (c) only a very small interval of time elapsed between the starting of the rotation apparatus and the ending of the exposure to the comparison light. In the path of the standard light, back of the exposure disc was interposed a shutter. The exposure to the standard light was begun by raising this shutter. It was ended, as indicated above, by the exposure disc. That is, in making a determination, the shutter was raised and, six seconds later, the rotation of the exposure disc was begun, the sectors of which gave the exposure to the comparison light and shut off both lights simultaneously. From this it will be seen that the actual time of exposure to the standard light was in each case six seconds plus the small fraction of a second between the starting of the exposure disc and the ending of the exposure to the comparison light. This increment varied so little from determination to determination, however, that it was considered to have a negligible effect on the apparent brightness of the standard light after six seconds of adaptation. Moreover, for reasons to be stated in the following paragraphs, no attempt will be made to compare the results obtained by this method with those obtained by other methods for more than one item, namely, the time required for the sensation to reach its maximum. This it will be seen lessens considerably the chance of the above conditions of exposure operating as a significant variable factor.

In the historical discussion, it will be remembered, two serious criticisms were made of this method, as used by Broca and Sulzer. (1) The attempt to rate sensation for comparative purposes in photometric units is fundamentally wrong, for the photometric unit is a unit of light intensity, not of sensation. Two surfaces, for example, illuminated, respectively, by four and one foot-candles of light do not arouse sensations which sustain to each other the ratio of four to one, nor have we any knowledge of what ratio they do sustain to each other. A curve plotted to the just noticeable changes in the intensity of sensation gives, as we have already pointed out, a much better notion of how the sensation rises toward its maximum with different times of action of light on the eye. Since all of our methods are based on the latter type of rating the rise of sensation and since there seems to be no way of passing from one system of values to the other, it is clearly not possible to compare the results obtained by this method and by our own methods on any point but the time required for sensation to reach its maximum. A broader comparison is not allowable for still another reason. This reason is involved in our second point of criticism. (2) Broca and Sulzer did not give the eye the same time of exposure to the standard light either in a given series or from series to series. And even had they made the exposure a constant, as we have done in our use of the method, a fair comparison still could not be made of the values obtained in different parts of the rise; for in determining these values different intensities of standard light have to be used, and the rate of decay of sensation, even for the same length of exposure, is not the same for different intensities of light. This, it is obvious, would vitiate the scale of ratings and make a false showing of comparative values. A comparison does seem permissible, however, so long as the use of only one intensity of standard light is involved in making the determination, and care is exercised to keep the time of exposure of the eye to this light constant. As has already been seen, these conditions were not satisfied by Broca and Sulzer in any part of their work, and have been satisfied in our work only with regard to one feature of the general problem, namely, the determination of the time required for the sensation to reach its maximum. The values obtained for this time, alone, can be used, therefore, in our inter-comparisons of results.

The results of our work with this method are given in Table VIII and Figure XIII. Since the method has been used only for comparative purposes, the determinations have been made for only one intensity of light. It would be more desirable could several intensities have been used in the work of inter-compari-

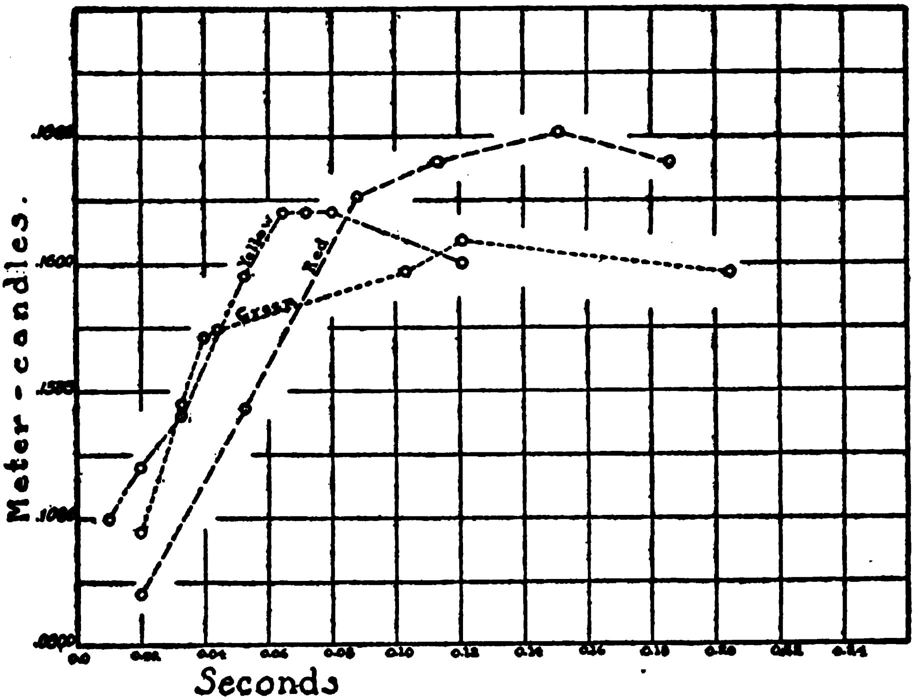


FIGURE XIII

Showing the results of a determination of the rise of sensation by Broca and Sulzer's method as modified by the author. The lights used to produce the sensation under investigation were made photometrically equal at 0.151 meter-candle.

son; but the amount of work involved in making this determination, especially in case of the older methods, was prohibitive of a broader study. For the intensity employed, the time required for the sensation to reach its maximum was for the yellow light 0.065-0.080 seconds; for the green, 0.121 seconds; and for the red, 0.151 seconds. A comparison of these values with those obtained by the other methods for the same intensity of light shows, it will be noted in the tables that follow, a close agreement.

TABLE VIII

Showing the results of a determination of the rise of sensation by Broca and Sulzer's method as modified by the author. The lights used to produce the sensation under consideration (comparison lights) were made photometrically equal at 0.151 meter-candle. In making the determinations the intensity of the comparison light was kept constant and the intervals of exposure were varied; and the intervals of exposure of the standard light was kept constant and its intensity was varied.

Stimulus	*Time of exposure of comparison light in seconds*	*Ratio of intensity of standard and comparison light when judged equal in brightness*	*Intensity of standard light in meter-candles when judged equal in brightness to comparison light*	*Time required for sensation to rise to its maximum value*
Red $686\mu\mu$	0.020	1 : 0.60	0.0900	
	0.053	1 : 0.90	0.1350	
	0.088	1 : 1.17	0.1755	
	0.113	1 : 1.21	0.1815	
	0.151	1 : 1.26	0.1880	0.151
	0.186	1 : 1.21	0.1815	
Yellow $580\mu\mu$	0.100	1 : 0.71	0.1065	
	0.020	1 : 0.78	0.1170	
	0.033	1 : 0.85	0.1275	
	0.053	1 : 1.05	0.1575	
	0.065	1 : 1.14	0.1710	0.065
	0.072	1 : 1.14	0.1710	
	0.080	1 : 1.14	0.1710	
	0.121	1 : 1.07	0.1605	
Green $511\mu\mu$	0.020	1 : 0.68	0.1020	
	0.033	1 : 0.87	0.1305	
	0.040	1 : 0.96	0.1440	
	0.044	1 : 0.98	0.1470	
	0.121	1 : 1.10	0.1650	0.121
	0.130	1 : 1.06	0.1590	
	0.205	1 : 1.06	0.1590	

(b) *Exner's Method.* In case of this method the author has adopted the procedure originated and used by Exner in 1870 with one modification,—the judgment of the relative brightness of the two lights was made of the original stimuli at the moment of their disappearance. That is a comparison of the positive after-images was not made, as Exner states was done by him.[40] In this method, it will be remembered, the two stimuli are of equal intensity; but the exposure of one is started before the exposure of the other. From a consideration of these conditions, it is obvious that the sensations aroused by the two lights can not be judged equal at the exact maximum for either. The sensation aroused by the shorter exposure will not have reached its maximum; while the sensation aroused by the longer exposure will have passed beyond its maximum. That is, the judgment of equality will be made when the loss in brightness due to the decay of the one is equal to the deficiency due to the under-development of the other. Now by variations, from small to great, in the differences in the lengths of exposures of the two lights, a series of values can be found which lie by increasingly greater amounts on either side of the maximum. The following cases are possible. (a) If the difference is very small the two stimuli will be judged equal for all, or nearly all, of the lengths of time for which they can be exposed. (b) If the difference is made larger, the stimuli will be judged equal for a range of exposures including the maximum and a range on either side of the maximum, the size of the range depending on the magnitude of the difference of the two exposures. And (c) if the difference is made still larger the stimuli will be judged equal for each value of the difference at only one length of exposure for either of the two stimuli. In this case the sensation aroused by the light first exposed will have passed its maximum value and the sensation aroused by the light last exposed will not have reached its maximum value. The maximum would be approximately the mean of these two values provided the rate of decay of sensation is equal, or nearly so, for the intervals in question, to the rate of development. This for short intervals just before and just after the maximum is reached, does not seem to be far from true. One way of estimating the time required to obtain the maximum, then would be to take the mean of these exposures arousing equal values in sensation whose differences are small enough, on the one hand, to fall within the region of equal rates of rise and decay about the maximum and large enough, on the other hand, that the judgment of equality can be given for only one or, at most only a very small range of exposures. Another way would be to assume with Exner that when the difference of the time of exposure of the two lights is as small as is possible, under the conditions necessary to the experiment the interval for which the judgment of equality is made is, for the first exposed, the time required for it to arouse its maximum response. But taken literally this assumption is not correct, for as we have already pointed out, the difference in the time of exposure to the two lights may be made so small that the two sensations would be judged equal for all or the greater number of values of exposure that might be used; and (b) the time of exposure for neither light can be the optimum time of exposure for that light. Both on account of its indefiniteness and relatively greater inaccuracy, no attempt has been made to apply Exner's method for estimating any point but the maximum.

---

[40] The author, like others who have tried this method, was unable to make any judgment in terms of the after-response, in fact, no definite positive or negative after-images could be detected.

Our procedure in carrying out this method was as follows: A given difference in the values of the open sectors of the double exposure disc for the two stimuli, was chosen. The lengths of exposure of the two stimuli were then varied until values were obtained for which the light, whose exposure was begun first, was judged in turn lighter, equal, and darker than the other light. Another constant difference of open sector[41] was then chosen and the series was repeated.

---

[41] On account of the difference in the inertia of the discs of the exposure apparatus when set with different total open sectors equal difference in open sector did not mean equal difference in exposure time. The author found it necessary to measure the time for each setting separately. This procedure allowed the experimenter to find points on opposite sides of the maximum which were judged equal, and therefore to reckon the maximum.

The results of this method are given in Table IX. They are not given, however, in the order in which they were obtained; but have been arranged for the convenience of the reader into groups in which the light longer exposed was judged, respectively, lighter, equal, and darker than the light exposed for the shorter time. Our method of estimating the optimum length of exposure can, perhaps, be made clearer by an example from the table. For this purpose the results for only one color need be considered, namely, red. The table shows that, in case of this color, the sensations aroused by nine pairs of exposures were judged equal. In accord with our criteria stated above, three pairs of these were discarded—0.070 and 0.101 seconds, 0.170 and 0.192 seconds, and 0.140 and 0.170 seconds—because the difference in time of exposure between the members of the pairs in each case was too small, as shown by the other data in this table, to produce a just noticeable difference in sensation. That is, it is seen that in another place in the table the exposures, 0.140 and 0.070 seconds, for example, gave a judgment of lighter in favor of the longer exposure. This shows that 0.101 is less than the optimum value of exposure for red and that the judgment of equality was given in the first and third of the above cases, not because the optimum exposure was attained, but because the difference between the two exposures,—0.03 seconds in each case,—was too small to cause a just noticeable difference in sensation. Since all the work has shown that in general, within the range of exposures used, a greater difference in length of exposure is required to give a just noticeable difference in sensation as the order of magnitude of both exposures is increased, it can hardly be assumed that the difference in the length of the two exposures in the second case, 0.022 seconds, was sufficient to cause a just noticeable difference in sensation. It does not seem fair, therefore, to use this result, in the estimation of the average either, although so clear a reason for discarding it is not present as was the case for the other pairs of exposures. That is, a judgment of equality can not be considered as evidence that the critical values needed for the estimation of the optimum exposure have been attained when the judgment might

Table IX

Showing the results of a determination of the rise of sensation by Exner's methods modified by the author. The lights used to produce the sensation under investigation were made photometrically equal at 0.057 meter-candles. The light the exposure of which was begun first, is referred to in the table as the standard light, the other is referred to as the comparison light.

Stimulus	*Time of exposure in seconds*		*Difference in time of exposure of the two lights.*	*Judgment of brightness of standard light in terms of comparison light.*	*Mean values of exposures needed to make the two lights appear equally bright.*	*Time required for sensation to reach its maximum.*
	Standard light.	*Comparison light.*				
Red 686$\mu\mu$	0.072	0.050	0.022	lighter		
	0.140	0.070	0.070	“		
	0.178	0.128	0.050	“		
	0.182	0.132	0.050	“		
	0.188	0.136	0.052	“		
	0.192	0.096	0.096	“		
	0.101	0.070	0.031	equal	(omitted)*	
	0.170	0.140	0.030	“	“	
	0.192	0.170	0.022	“	“	
	0.192	0.156	0.036	“	0.174	
	0.182	0.140	0.042	“	0.161	
	0.192	0.120	0.072	“	0.156	0.161
	0.198	0.132	0.066	“	0.165	
	0.198	0.111	0.087	“	0.154	
	0.203	0.120	0.083	“	0.161	
	0.203	0.140	0.063	darker		
	0.207	0.125	0.082	“		
	0.203	0.170	0.033	“		
	0.211	0.178	0.033	“		
Yellow 580$\mu\mu$	0.101	0.044	0.057	lighter		
	0.120	0.070	0.050	“		
	0.124	0.075	0.049	“		
	0.101	0.075	0.026	“		
	0.124	0.101	0.023	“		
	0.064	0.038	0.026	“		
	0.128	0.080	0.048	equal	0.104	
	0.134	0.085	0.049	“	0.109	
	0.106	0.080	0.026	“	0.093	
	0.110	0.085	0.025	“	0.097	0.099
	0.136	0.090	0.046	“	0.113	
	0.120	0.096	0.024	“	0.108	
	0.115	0.075	0.040	“	0.095	

TABLE IX (continued)

Stimulus	*Time of exposure in seconds*		*Difference in time of exposure of the two lights.*	*Judgment of brightness of standard light in terms of comparison light.*	*Mean values of exposures needed to make the two lights appear equally bright.*	*Time required for sensation to reach its maximum.*
	Standard light.	*Comparison light.*				
	0.085	0.070	0.015	equal	(omitted)*	
	0.115	0.101	0.014	"	"	
	0.140	0.096	0.044	darker		
	0.146	0.101	0.045	"		
Green 511$\mu\mu$	0.170	0.120	0.050	lighter		
	0.182	0.188	0.024	"		
	0.192	0.120	0.072	"		
	0.203	0.140	0.063	"		
	0.215	0.158	0.057	"		
	0.224	0.140	0.084	"		
	0.250	0.140	0.110	"		
	0.203	0.170	0.033	equal	0.186	
	0.224	0.170	0.054	"	0.197	
	0.236	0.170	0.066	"	0.203	0.193
	0.261	0.150	0.091	"	0.205	
	0.192	0.160	0.032	"	0.176	
	0.271	0.192	0.079	darker		
Blue 463$\mu\mu$	0.170	0.120	0.050	lighter		
	0.203	0.170	0.033	"		
	0.215	0.158	0.057	"		
	0.224	0.140	0.084	"		
	0.231	0.152	0.079	"		
	0.235	0.165	0.070	"		
	0.211	0.178	0.034	equal	0.194	
	0.224	0.192	0.032	"	0.208	
	0.250	0.224	0.026	"	0.237	
	0.236	0.203	0.033	"	0.218	
	0.228	0.196	0.032	"	0.212	0.210
	0.224	0.170	0.054	"	0.197	
	0.236	0.192	0.044	"	0.214	
	0.236	0.170	0.066	"	0.203	
	0.240	0.178	0.068	"	0.209	
	0.254	0.228	0.026	darker		
	0.250	0.203	0.047	"		
	0.240	0.196	0.044	"		
	0.248	0.188	0.060	"		

* These numbers were omitted from the final calculation of the time required for the sensation to reach its maximum value for reasons stated in the text. (See p. 72.)

just as well have been due to the insufficient difference in the length of the two exposures to give any but a judgment of equality. The optimum time of exposure, was estimated, therefore, from the six remaining pairs of exposures that gave the judgment of equality. So estimated, the optimum for red of the intensity and range of wave-lengths used was found to be 0.161 seconds. By a similar method of estimation the optimum for yellow, green, and blue were, respectively 0.099 seconds, 0.193 seconds, 0.210 seconds. These values, it will be observed, agree very well with the results obtained by the other methods of determining the maximum.

(c) *Kunkel's Method.* In applying this method, it will be remembered that two conditions are essential. (1) The lights used must be of different intensities. One, the standard light, must be a great deal stronger than the other, the comparison light, and, if the method is to accomplish its purpose, the sensation aroused by the stronger light must, as assumed by Exner, rise more rapidly towards its maximum than that aroused by the weaker light. The intervals of exposure of the two lights must be so regulated, however, that the sensation aroused by the weaker light reaches its maximum before that aroused by the stronger light. And (2) a variable and not a constant difference in the time of exposure of the two lights must be employed in each series of determinations. That is, the apparatus is so arranged that the exposure to the two lights end at the same time. A given very small interval of exposure is chosen for the comparison light. Then a time of exposure is found for the standard light which causes it just to match the comparison light in apparent brightness. This procedure is repeated for a graded series of exposures for the comparison light. From this series that value of exposure of the comparison light is selected as optimum, which is matched in its effect on sensation by the longest in the series of exposures given to the standard light. (See more detailed description of method, p. 26.) In our use of this method a standard light was chosen, approximately twice as intense as the comparison light.[42] A short initial exposure then was given to the standard light and the exposure of the comparison light was varied until at the moment of disappearance it was judged lighter than the standard light. The length of exposure of the standard was then increased until it was judged equal in brightness to the comparison. This procedure was continued until an exposure was found for the standard light which aroused a sensation which could be matched, but never exceeded, by the sensation aroused by the comparison light, however, much its exposure was increased. In accord with the principal of the method, this exposure was chosen as the optimum for the comparison light.

---

[42] In striving for precision by this method, two factors must be taken into

Table X shows a typical set of results for this method. They are published complete with the exception that exposures of the standard light which were too short to give any judgments of equality are omitted. In the first column is given the stimuli with the region of the spectrum from which it was taken; in the second and third, the intervals of exposure of the standard and comparison lights, respectively; and in the fourth column, the judgment of the comparison light in terms of the standard. A great help in understanding the application of this method can be had by comparing columns 2, 3 and 4.

The results show that, although the intervals required for the colors to rise to their maximum are in the same order as for the other methods, the actual values of the exposures are in each case slightly greater. This is true consistently for all of the colors. A reason for this may be found, perhaps, in the discussion of methods given earlier in the paper. (See discussion of Dürr's method, p. 30.) That is, when a standard light very much

---

account. (1) The more intense is the standard light, the more rapid is the rise of the sensation which it arouses. With the greater intensities of standard light there is, therefore, a greater variation of apparent brightness per unit of time. If there were no counteracting influences this should lead to a greater precision in isolating the optimum exposure for the comparison light. That is, a smaller range of exposures could give a sensation which matches the sensation aroused by any one exposure of the standard light. But (2) if too intense a light is chosen for the standard, its time of exposure must be made very short to be available in the series, and the judgment is thereby rendered more difficult. For the author the best results were obtained with a standard light of about twice the intensity of the comparison light.

TABLE X

Showing the results of a determination of the rise of sensation by Kunkel's method. The lights used to produce the sensations under investigation were made photometrically equal at 0.057 meter-candles.

Stimulus	*Time of exposure of standard light in seconds*	*Time of exposure of comparison light in seconds*	*Judgment of brightness of comparison light in terms of standard light*	*Time required for sensation to reach its maximum value*
Red 686μμ	0.038	0.120	lighter	
	0.054	0.120	equal	
	0.054	0.170	lighter	
	0.070	0.164	darker	
	0.070	0.170	equal	
	0.070	0.203	"	0.196
	0.070	0.217	"	
	0.070	0.224	darker	
	0.070	0.250	"	
Yellow 580μμ	0.026	0.070	lighter	
	0.032	0.070	equal	
	0.032	0.096	lighter	
	0.038	0.096	lighter	
	0.042	0.096	lighter	
	0.044	0.096	equal	
	0.044	0.120	equal	
	0.044	0.115	lighter	
	0.047	0.120	darker	
	0.047	0.115	equal	0.115
	0.047	0.096	darker	
Green 511μμ	0.070	0.170	lighter	
	0.080	0.170	equal	
	0.080	0.203	lighter	
	0.090	0.203	lighter	
	0.096	0.224	darker	
	0.096	0.216	equal	0.207
	0.096	0.198	equal	
	0.096	0.196	darker	

stronger than the comparison is used, the result is apparently to lengthen the time necessary for the sensation to reach its maximum; but this lengthening is less where the post-exposure is white instead of black—a fact which seems to show that the positive after-image has an effect on the determination of the kind described above. Here, however, this effect does not seem so pronounced as it was with the method used by Dürr. Two reasons may, probably, be assigned for this: (a) the ratio of intensity of the standard and comparison lights is not so large; and (2) the time of exposure of the standard light is much shorter. Both of these factors, according to the results of Franz (given earlier in this paper, p. 33) would tend to lessen the effect of the positive after-image; still it is very conceivable that the positive after-image may have an effect when the judgment has to be made just at the moment both stimuli disappear, as is the case in work of this kind. In any event the lack of agreement falls within a just noticeable difference, an amount which can hardly be considered of great significance since the effect on sensation of a smaller difference in exposure could not be detected. In fact, the author has sought an explanation of the disagreement only because it was consistently in the same direction.

(2) THE NEWER METHODS

(a) *Method 1.* The first step in this method, it will be remembered, is to establish a just noticeable difference series for each color to serve as a series of graded standard sensations against which to match the sensation under investigation at the various stages in its rise to the maximum. In determining this series of just noticeable differences, the time of exposure to the standard light was kept constant at 0.261 seconds and the changes in sensation were produced by varying the widths of the two halves of the collimator slit. In doing this the two halves served in alternation as standard and comparison until a series of sufficient length was obtained, *i.e.* the lower half, for example, was held constant and the upper half was varied until a just noticeable difference in sensation was produced; and then the upper half was held constant at the last setting of the previous determination and the lower half was varied, etc. until the end of the series was reached. The sensation under investigation was then compared with the different members of the series, the object being to find a time of exposure to the comparison light that would arouse a sensation which would match in turn each just noticeably different step in the series until the maximum was reached. For example, a yellow stimulus of 0.151 meter candles, when its time of exposure was 0.051 seconds gave a sensation which was judged equal to the fifth just noticeable difference in the series. When a point is reached beyond which no higher step in the series can be matched by making any further increase in the time of exposure to the comparison light, it is concluded that the maximum response to that light has been attained. Two intensities of light were used with this method for each range of wave-length, —0.057 meter candles and 0.151 meter candles. The results of the determinations are given in Tables XI, and XII, and figures XIV and XV. It will be noted in these tables that time has not been taken to match each just noticeable difference in the standard series.

TABLE XI

Showing the results of a determination of the rise of sensation by Method 1. The lights used to produce the sensation were made photometrically equal at 0.057 meter-candles.

Stimulus	*Time of exposure of comparison light in seconds*	*Just noticeable difference in standard series to which the sensation aroused by the comparison light is judged equal*	*Time required for sensation to rise to its maximum value*
Red $686\mu\mu$	0.015	1	
	0.035	3	
	0.060	4	
	0.117	6	
	0.160	7	0.160
	0.185	6	
Yellow $580\mu\mu$	0.020	2	
	0.060	5	
	0.085	6	
	0.105	7	0.105
	0.162	5	
Green $511\mu\mu$	0.020	1	
	0.035	2	
	0.060	4	
	0.105	5	
	0.183	7	0.183
	0.262	6	

TABLE XII

Showing the results of a determination of the rise of sensation by Method 1. The lights used to produce the sensation were made photometrically equal at 0.151 meter-candles.

Stimulus	*Time of exposure of comparison light in seconds*	*Just noticeable difference in standard series to which the sensation aroused by the comparison light is judged equal*	*Time required for sensation to rise to its maximum value*
Red $686\mu\mu$	0.030	2	
	0.051	4	
	0.085	6	
	0.105	7	
	0.144	8	0.144
	0.262	7	
Yellow $580\mu\mu$	0.020	2	
	0.029	4	
	0.035	5	
	0.060	6	
	0.095	8	0.095
	0.183	7	
Green $511\mu\mu$	0.020	1	
	0.030	3	
	0.065	6	
	0.105	8	
	0.144	9	0.144
	0.262	8	

(b) *Method 2*. Method 2 is an indirect method and has not the same *a priori* claim to sureness of principle as has Methods 1 and 3. The method is based on the assumption that the time of exposure that gives the greatest number of just noticeable changes from the zero of sensation, is the time most favorable for light of that intensity to produce sensation, in other words is the time which just allows the sensation to reach its maximum. Besides determining for us the time of exposure which gives the maximum value of the sensation, the method also provides a means of rating the sensation values for different times of exposure in terms of just noticeable differences from the zero. The application of the method is as follows: Beginning with a very short time of exposure a just noticeable difference series is de-

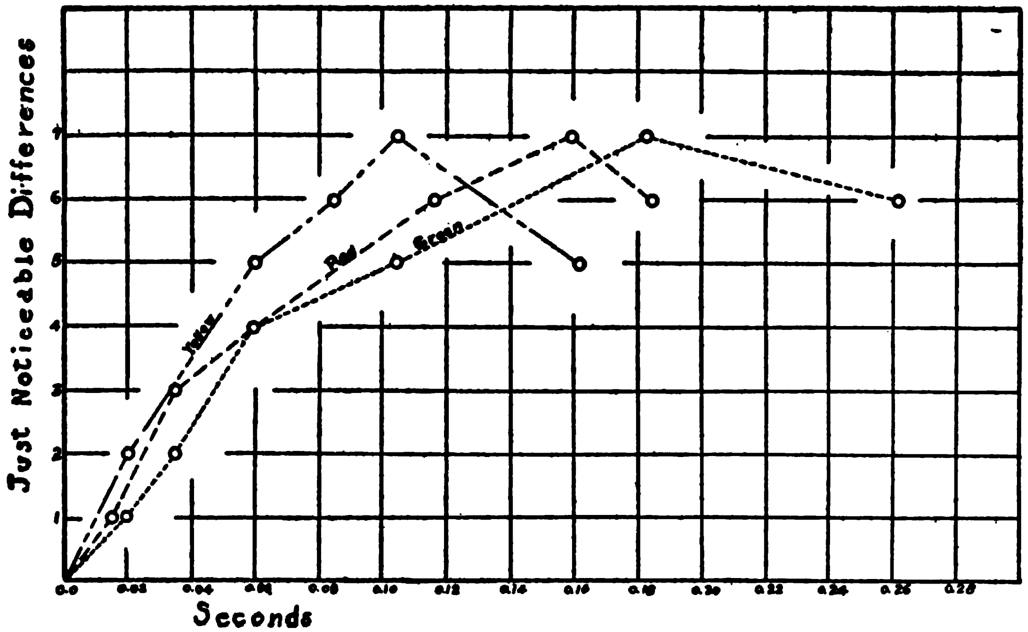


FIGURE XIV

Showing the results of a determination of the rise of sensation by Method 1. The lights used to produce the sensations under investigation were made photometrically equal at 0.057 meter-candles.

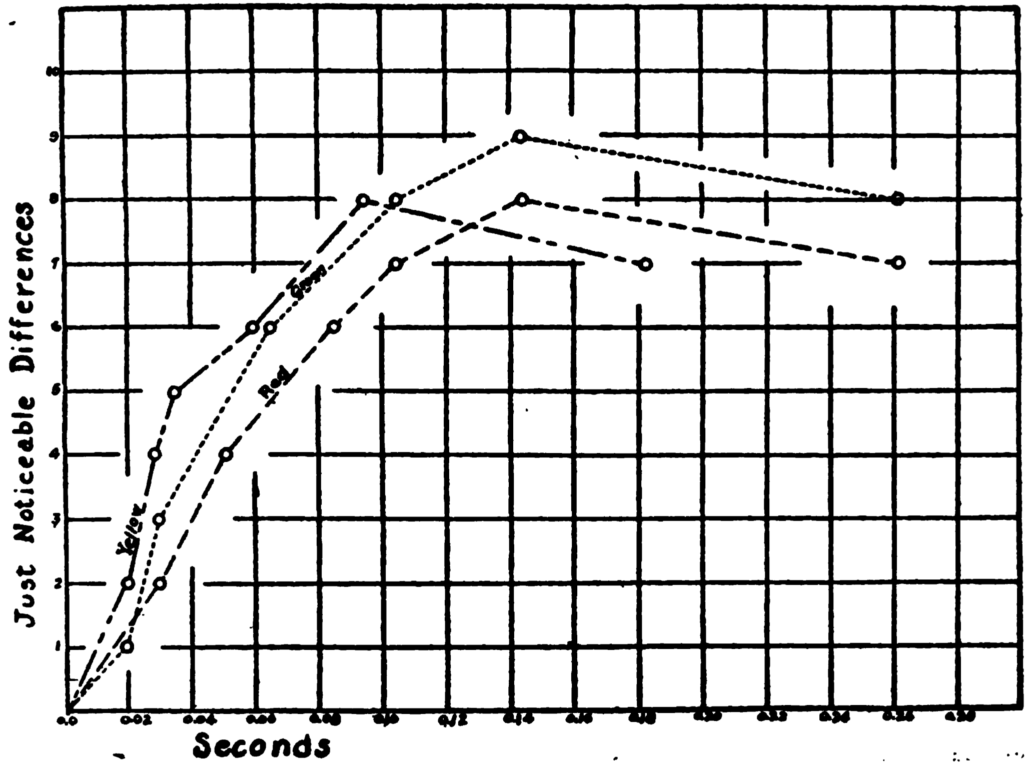


FIGURE XV

Showing the results of a determination of the rise of sensation by Method 1. The lights used to produce the sensations under investigation were made photometrically equal at 0.151 meter-candles.

termined. This determination is repeated with a slightly longer time of exposure. The determinations are continued, with small increases in the time of exposure, until one is found that gives the greatest number of just noticeable differences from the zero of sensation for the light in question. This time of exposure is selected as the optimum, and the comparative values of the other exposures for producing sensation are rated by the number of just noticeable differences that were produced by each. The small changes of intensity, needed for making the just noticeable difference determinations, were obtained by means of the double collimator slit, previously described. The procedure in making the determinations was the same as used in Method 1. The results obtained by the method are shown in Table XIII and figure XVI. Since an entire just noticeable difference series has to be determined for each interval of exposure, the time and labor involved in using the method is great. For this reason it has been employed in this investigation only as far as was needed to afford a comparison with the results of other methods. A comparison of results with those of Methods 1 and 3 shows the following points of similarity: the number of just noticeable differences between the zero and the maximum is the same; the order of rate of rise for the different colors, also, is the same; and the agreement in the lengths of exposure that are required to give the maximum of sensation, is fairly close.

Attention may be called in passing to a difficulty in applying the method which is, perhaps, responsible for the lack of smoothness of the curves. Obviously the setting for a just noticeable difference does not, for every value of exposure that may be selected, coincide with the slit width representing the intensity of light used. Since the just noticeable difference can not be fractioned, the next lower integral value was taken in each case for the number of just noticeable differences produced by the given time of exposure. The cases in which the difficulty occurred, necessitated the correlation of the given number of just noticeable differences with a time of exposure a little longer than the correct value. With time and patience it would, of course, have been possible to have determined with minute exactness the interval needed just to give the number of just noticeable differences in question. This should have resulted in smoother curves. The results. moreover, would have been more strictly comparable with those obtained by other methods.

TABLE XIII

Showing the results of a determination of the rise of sensation by Method 2. The light used to produce the sensations under investigation were made photometrically equal at 0.057 meter-candles.

Stimulus	*Time of exposure of lights in seconds*	*Number of just noticeable differences in sensation produced by increasing the stimulus from 0 to 0.057 meter-candles*	*Time required for sensation to rise to its maximum value*
Red $686\mu\mu$	0.015	1	
	0.065	4	
	0.137	6+*	
	0.161	7+	0.161
	0.260	6	
Yellow $580\mu\mu$	0.010	1	
	0.065	5	
	0.103	7	0.103
	0.161	5	
Green $511\mu\mu$	0.020	1	
	0.065	3+	
	0.121	5	
	0.161	6	
	0.186	7+	0.186
	0.262	6+	
Blue $463\mu\mu$	0.015	1	
	0.065	3+	
	0.121	4+	
	0.140	5	
	0.205	7	0.205
	0.262	6	

(c) *Method 3*. In this as in the former methods, it will be remembered, the sensation in its rise towards the maximum is graded in terms of just noticeable differences; but that just noticeable differences are produced by changes in the time of exposure, the intensity of the light being kept constant throughout.

---

* The sign indicates that the given just noticeable difference ended before 0.057 meter-candles was reached, but that the next just noticeable difference carried it beyond this intensity.

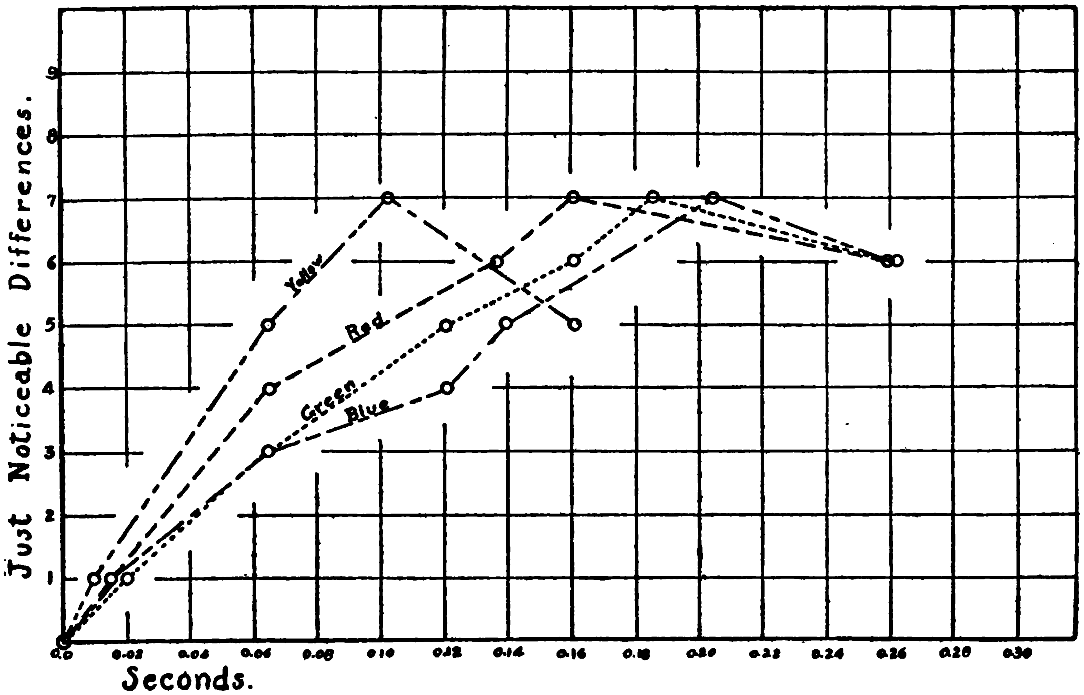


FIGURE XVI

Showing the results of a determination of the rise of sensation by Method 2. The lights used to produce the sensations under investigation were made photometrically equal at 0.057 meter-candles.

The procedure in applying the method is as follows: The upper and lower halves of the double slit of the collimator were set at a width giving the desired intensity of light and kept constant throughout the series of observations. One set of discs of the exposure apparatus was closed completely and the other was opened by a series of adjustments until a just noticeable sensation was aroused. For the next observation, this opening was held constant and the first set of discs was opened until the light admitted was judged just noticeably brighter than the other light. The series was continued, first one and then the other light being made the standard for the new observation, until a value of exposure was attained, no further increase of which caused a noticeable increase in brightness. This was accepted as the optimum, and the sensations produced by the other exposures were rated in terms of their just noticeable differences from the zero.

Because of a sureness of principle equal to, if not greater than any of the others, and a greater ease and convenience of application, a wider range of work was carried out with this method than with any of the others. Three intensities of light were used; and in addition to the four colored lights, a white light, formed by the synthesis of the spectrum from which the colors were taken. The photometric values of these intensities are 0.057, 0.151, and 1.21 meter-candles. The radiometric values are given in Table VII, p. 65.

The results obtained by this method are given in Tables XIV, XV, and XVI, and Figures XVII, XVIII, XIX. These results show that the rates of rise of sensation for the different lights at the intensities used are in the following orders. For an illumination of 0.057 meter-candles the order is yellow, red, green, blue, and white—the maximum for yellow, by an exposure of 0.10 seconds, the maximum for red, by an exposure of 0.164 seconds, the maximum for green at 0.190 seconds, the maximum for blue by an exposure of 0.210 seconds, the maximum for white by an exposure of 0.216 seconds. For an illumination of 0.151 meter-candles the order of rise is different for different parts of the curves. That is, in passing from the lowest to the highest of the three intensities, there is a marked difference in the order of rise of the colors amounting to a reversal for the pairs of colors red and yellow, and blue and green. In case of the intermediate intensity, as might perhaps be expected, the tendency in the low part of the curves seems to be roughly following the order of rise that was shown in the curves for the lower intensity; and in the upper part of the curves, to follow the tendency shown in the curves for the higher intensity.

In case of this intermediate intensity, the maximum for yellow was produced by an exposure of 0.086 seconds; for blue, by an exposure of 0.134 seconds; for green, by an exposure of 0.146 seconds; for red, by an exposure of 0.148 seconds, and for white, by an exposure of 0.184 seconds. For the highest intensity 1.211 meter-candles, blue could not be obtained with the apparatus used. The order of rise for the remaining lights, up to within two just noticeable differences of the maximum for green, is green, yellow, white and red. At this point green lags behind the other colors. The maximum for yellow, we find, is produced by an exposure 0.088 seconds; for red, by an exposure of 0.100 seconds; for white, by an exposure of 0.105 seconds; and for green, by an exposure of 0.136 seconds. It will be noted in a consideration of these tables and curves that the lights judged photometrically equal by the equality of brightness method have the same number of just noticeable differences between the zero and the maximum, with the exception of green for the intermediate and highest intensity and white for the intermediate intensity. The additional just noticeable difference, found with these two lights in the cases mentioned, may be an individual peculiarity. In this connection it should be remembered, however, as we have stated in our introductory chapter, that owing to the different rates of rise and decay of sensation for lights of different composition, a photometric balance established for different wave-lengths can be considered as valid only for the particular lengths of exposure of the eye used in making the balance. That is, if a longer or shorter exposure should be used, there is no *a priori* reason to expect that the balance would be valid. Since the photometric balance for the above lights was made with a length of exposure different from the time required for each to give its maximum effect on sensation it is not strange from *a priori* considerations that a rating of these lights made with an exposure of the time required for each to give its maximum effect should be found to differ in one or more cases from the photometric rating. The shift in the order of rise for the colors with increase of intensity which these results show, may account for some of the variations in the results which have been obtained by different observers. In the work of previous investigators, it will be remembered, no care was taken to duplicate the intensities of light used by other investigators.

TABLE XIV

Showing the results of a determination of the rise of sensation by Method 3. The lights used to produce the sensation under investigation were made photometrically equal at 0.057 meter-candles.

	Time of exposure in seconds		*Judgment of brightness of comparison light in terms of standard light*	*Time required for sensation to rise to to its maximum value*
Stimulus	*Standard light*	*Comparison light*		
Red 686μμ	0.000	0.010	lighter	
	0.010	0.026	"	
	0.026	0.044	"	
	0.044	0.062	"	
	0.062	0.095	"	
	0.095	0.123	"	
	0.123	0.164	"	0.164
	0.164	0.210	darker	
Yellow 580μμ	0.000	0.005	lighter	
	0.005	0.015	"	
	0.015	0.026	"	
	0.026	0.042	"	
	0.042	0.056	"	
	0.056	0.080	"	
	0.080	0.103	"	0.103
	0.103	0.121	darker	
	0.121	0.137	"	
	0.137	0.164	"	
Green 511μμ	0.000	0.020	lighter	
	0.020	0.033	"	
	0.033	0.046	"	
	0.046	0.072	"	
	0.072	0.113	"	

TABLE XIV (Continued

Stimulus	*Time of exposure in seconds* *Standard light*	*Comparison light*	*Judgment of brightness of comparison light in terms of standard light*	*Time required for sensation to rise to to its maximum value*
	0.113	0.151	lighter	
	0.151	0.190	"	0.190
	0.190	0.245	darker	
Blue 463$\mu\mu$	0.000	0.010	lighter	
	0.010	0.026	"	
	0.026	0.044	"	
	0.044	0.072	"	
	0.072	0.113	"	
	0.113	0.156	"	
	0.156	0.210	"	0.210
	0.210	0.261	darker	
White	0.000	0.046	lighter	
	0.046	0.057	"	
	0.057	0.078	"	
	0.078	0.105	"	
	0.105	0.138	"	
	0.138	0.179	"	
	0.179	0.216	"	0.216
	0.216	0.271	darker	
	0.271	0.327	"	

TABLE XV

Showing the results of a determination of the rise of sensation by Method 3. The lights used to produce the sensation under investigation were made photometrically equal at 0.150 meter-candles.

	Time of exposure in seconds			
Stimulus	*Standard light*	*Comparison light*	*Judgment of brightness of comparison light in terms of standard light*	*Time required for sensation to rise to its maximum value in seconds*
Red 686$\mu\mu$	0.000	0.007	lighter	
	0.007	0.015	"	
	0.015	0.033	"	
	0.033	0.049	"	
	0.049	0.077	"	
	0.077	0.106	"	
	0.106	0.134	"	
	0.134	0.148	"	0.148
	0.148	0.184	darker	
	0.184	0.212	"	
	0.212	0.255	"	

TABLE XV (Continued)

Stimulus	*Time of exposure in seconds*		*Judgment of brightness of comparison light in terms of standard light*	*Time required for sensation to rise to its maximum value in seconds*
	Standard light	*Comparison light*		
Yellow 580μμ	0.000	0.007	lighter	
	0.007	0.015	"	
	0.015	0.024	"	
	0.024	0.033	"	
	0.033	0.044	"	
	0.044	0.056	"	
	0.056	0.068	"	
	0.068	0.086	"	0.086
	0.086	0.113	darker	
	0.113	0.137	"	
Green 511μμ	0.000	0.010	lighter	
	0.010	0.020	"	
	0.020	0.031	"	
	0.031	0.042	"	
	0.042	0.051	"	
	0.051	0.068	"	
	0.068	0.086	"	
	0.086	0.113	"	
	0.113	0.146	"	0.146
	0.146	0.206	darker	
	0.206	0.246	"	
Blue 463μμ	0.000	0.010	lighter	
	0.010	0.015	"	
	0.015	0.020	"	
	0.020	0.036	"	
	0.036	0.053	"	
	0.053	0.080	"	
	0.080	0.109	"	
	0.109	0.134	"	0.134
	0.134	0.148	darker	
	0.148	0.174	"	
	0.174	0.245	"	
White	0.000	0.034	lighter	
	0.034	0.046	"	
	0.046	0.055	"	
	0.055	0.066	"	
	0.066	0.080	"	
	0.080	0.098	"	
	0.098	0.124	"	
	0.124	0.151	"	
	0.151	0.184	"	0.184
	0.184	0.206	darker	

TABLE XVI

Showing the results of a determination of the rise of sensation by Method 3. The lights used to produce the sensation under investigation were made photometrically equal at 1.21 meter-candles.

	Time of exposure in seconds		*Judgment of of brightness of comparison light in terms of standard light*	*Time required for sensation to rise to its maximum value in seconds*
Stimulus	*Standard light*	*Comparison light*		
Red 686$\mu\mu$	0.000	0.003	lighter	
	0.003	0.007	"	
	0.007	0.012	"	
	0.012	0.016	"	
	0.016	0.021	"	
	0.021	0.026	"	
	0.026	0.033	"	
	0.033	0.044	"	
	0.044	0.052	"	
	0.052	0.063	"	
	0.063	0.078	"	
	0.078	0.090	"	
	0.090	0.100	"	0.100
	0.100	0.129	darker	
	0.129	0.153	"	
Yellow 580$\mu\mu$	0.000	0.002	lighter	
	0.002	0.004	"	
	0.004	0.007	"	
	0.007	0.010	"	
	0.010	0.015	"	
	0.015	0.020	"	
	0.020	0.026	"	
	0.026	0.033	"	
	0.033	0.043	"	
	0.043	0.055	"	
	0.055	0.069	"	
	0.069	0.086	"	
	0.086	0.088	"	0.088
	0.088	0.106	darker	
	0.106	0.126	"	
Green 511$\mu\mu$	0.000	0.002	lighter	
	0.002	0.004	"	
	0.004	0.006	"	
	0.006	0.009	"	
	0.009	0.011	"	
	0.011	0.013	"	
	0.013	0.016	"	

TABLE XVI (Continued)

Stimulus	*Time of exposure in seconds*		*Judgment of of brightness of comparison light in terms of standard light*	*Time required for sensation to rise to its maximum value in seconds*
	Standard light	*Comparison light*		
	0.010	0.022	lighter	
	0.022	0.034	"	
	0.034	0.046	"	
	0.046	0.060	"	
	0.060	0.072	"	
	0.072	0.112	"	
	0.112	0.136	"	0.136
	0.136	0.160	darker	
White	0.000	0.004	lighter	
	0.004	0.007	"	
	0.007	0.008	"	
	0.008	0.013	"	
	0.013	0.018	"	
	0.018	0.022	"	
	0.022	0.028	"	
	0.028	0.036	"	
	0.036	0.044	"	
	0.044	0.057	"	
	0.057	0.072	"	
	0.072	0.088	"	
	0.088	0.105	"	0.105
	0.105	0.122	darker	

As stated in a preceding section, one of the objects of the work has been an intercomparison of the results obtained, both by the new methods and such of the older ones as seemed possible, from the same observer working under the same experimental conditions and with the same wave-lengths and intensities of light. In order to facilitate such a comparison, the results for each color by all of the methods have been represented in the same chart. For the three new methods, curves have been plotted showing the rates of rise of sensation to the maximum in terms of just noticeable difference steps. This could not be done, of course, for the methods of Broca and Sulzer, Exner, and Kunkel, since these methods provide a means of determining only the time required for the sensation to rise to its maximum. All that could be done in those cases was to represent the

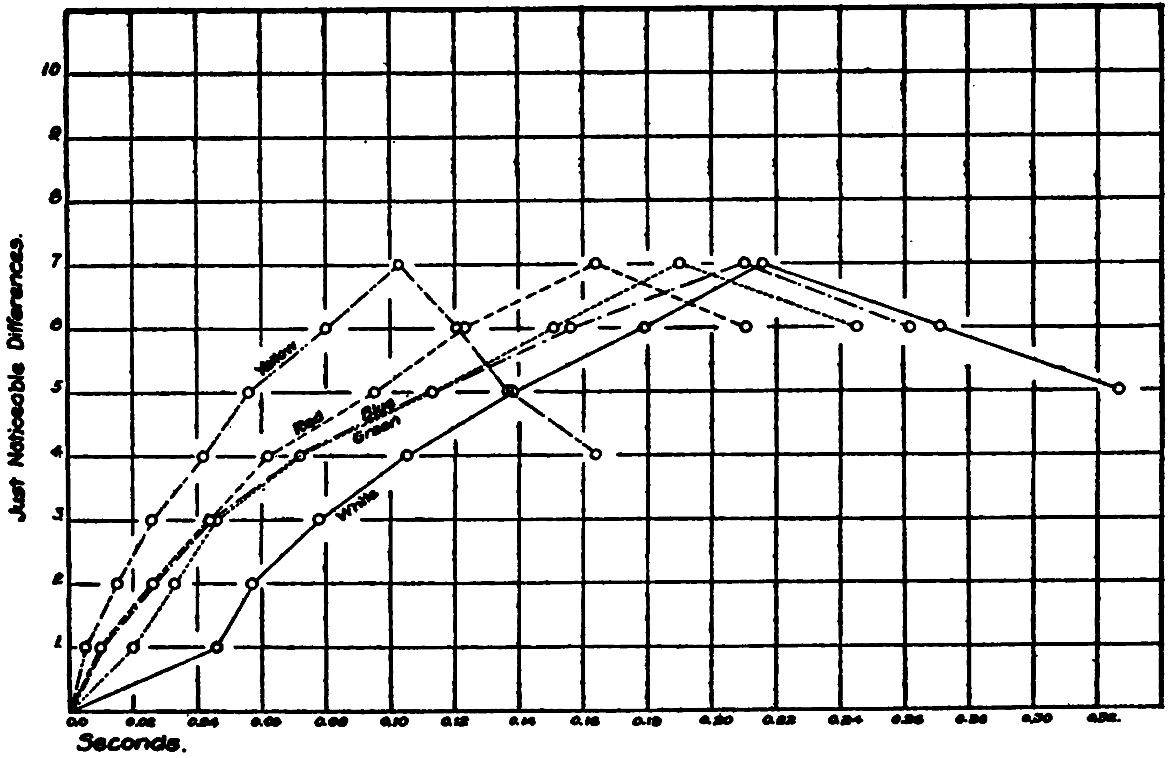


FIGURE XVII

Showing the results of a determination of the rise of sensation by Method 3. The lights used to produce the sensations under investigation were made photometrically equal at 0.057 meter-candles.

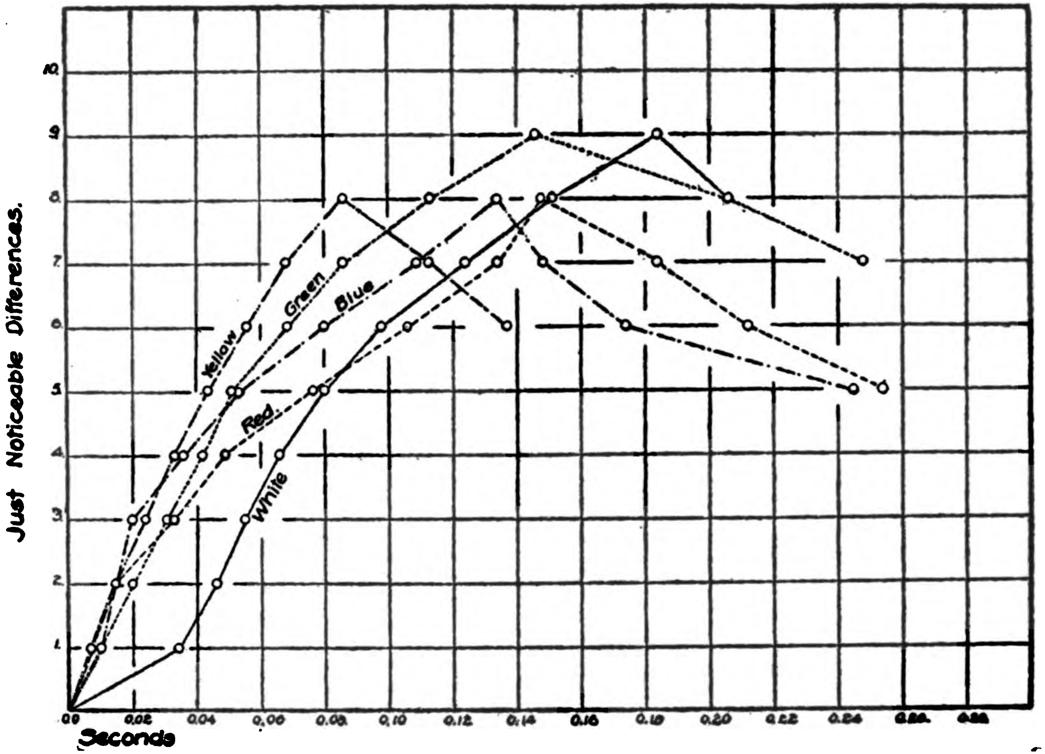


FIGURE XVIII

Showing the results of a determination of the rise of sensation by Method 3. The lights used to produce the sensations under investigation were made photometrically equal at 0.151 meter-candles.

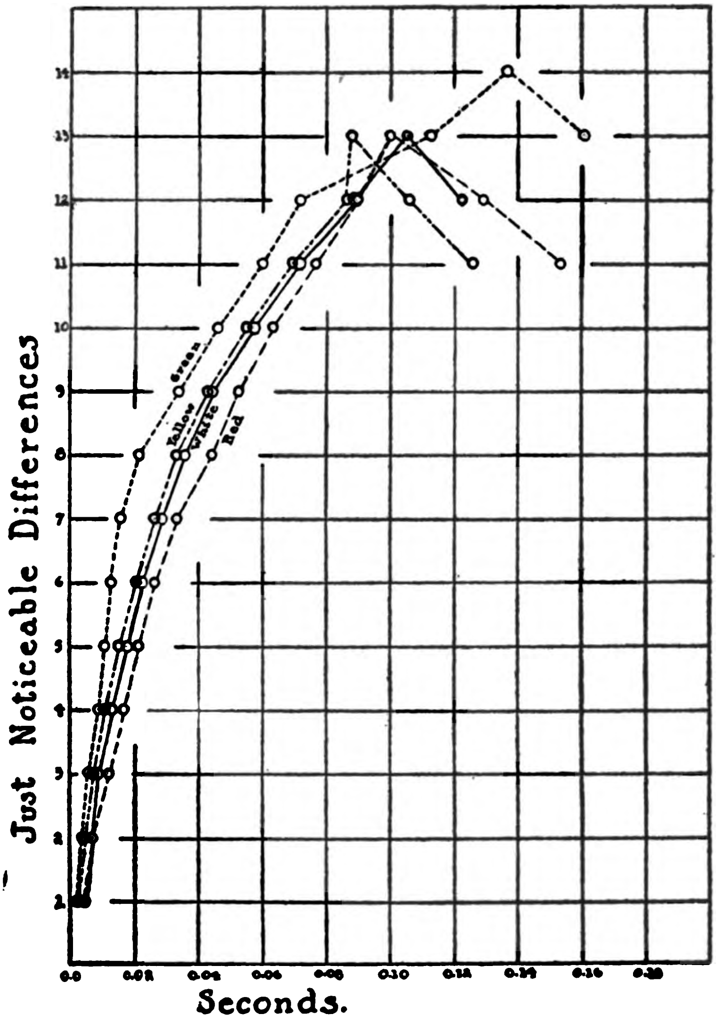


FIGURE XIX

Showing the results of a determination of the rise of sensation by Method 3. The lights to produce the sensations under investigation were made photometrically equal at 1.21 meter-candles.

optimum exposures at appropriate points in the charts. These charts are shown in Figures XX and XXI. An inspection of the charts shows a close agreement in the curves representing the rise of sensation for the three new methods. It will be seen also that the results obtained by the methods of Broca and Sulzer and Exner, in the form in which we have used them, show a close agreement with those obtained by the new methods, in the only way in which they can be compared. That is, they give nearly the same value for the optimum time of exposure. Kunkel's method, it will be noted, shows a poorer agreement, although the point of disagreement, as stated earlier in this paper, falls within the change in the exposure that would be required to produce a just noticeable difference in sensation. A much more serious objection to its use than its lack of agreement with other methods, is its infeasibility and cumbersomeness of application. Compared in this latter regard, which is a very important one, from the laboratory point of view, it is the least feasible of all the methods. Of the four remaining methods, the method of Broca and Sulzer is more convenient and feasible in practice than that of Exner; and Method 3 is the most and Method 2 the least convenient and feasible of the new methods. From the point of view of *a priori* sureness of principle Methods 1 and 3 have, in our opinion, the greatest claim to consideration; and of these Method 3 has a great advantage in convenience and feasibility of application. For general laboratory purposes, it is the most promising of any that we have taken into account in making this study.[48]

We have in the foregoing study attempted to make a comparison of the results of the methods of previous investigators with

---

[48] So far as convenience of use is concerned, it is only fair to say, however, that in any given case, the equipment of the laboratory must, of course, be taken into consideration. If there is any difference in the ease or accuracy with which the variations in time and the variations in intensity can be made with the apparatus available, this might, perhaps, determine the choice between the two methds. But even from the view-point of simplicity of the equipment needed, Method 3 has a decided advantage over Method 1 and 2. However, if the determination of the rise of sensation is to be made for a great number of intensities of light, Method 1 has a greater claim to consideration from the standpoint of relative quickness and speed of operation than is indicated above.

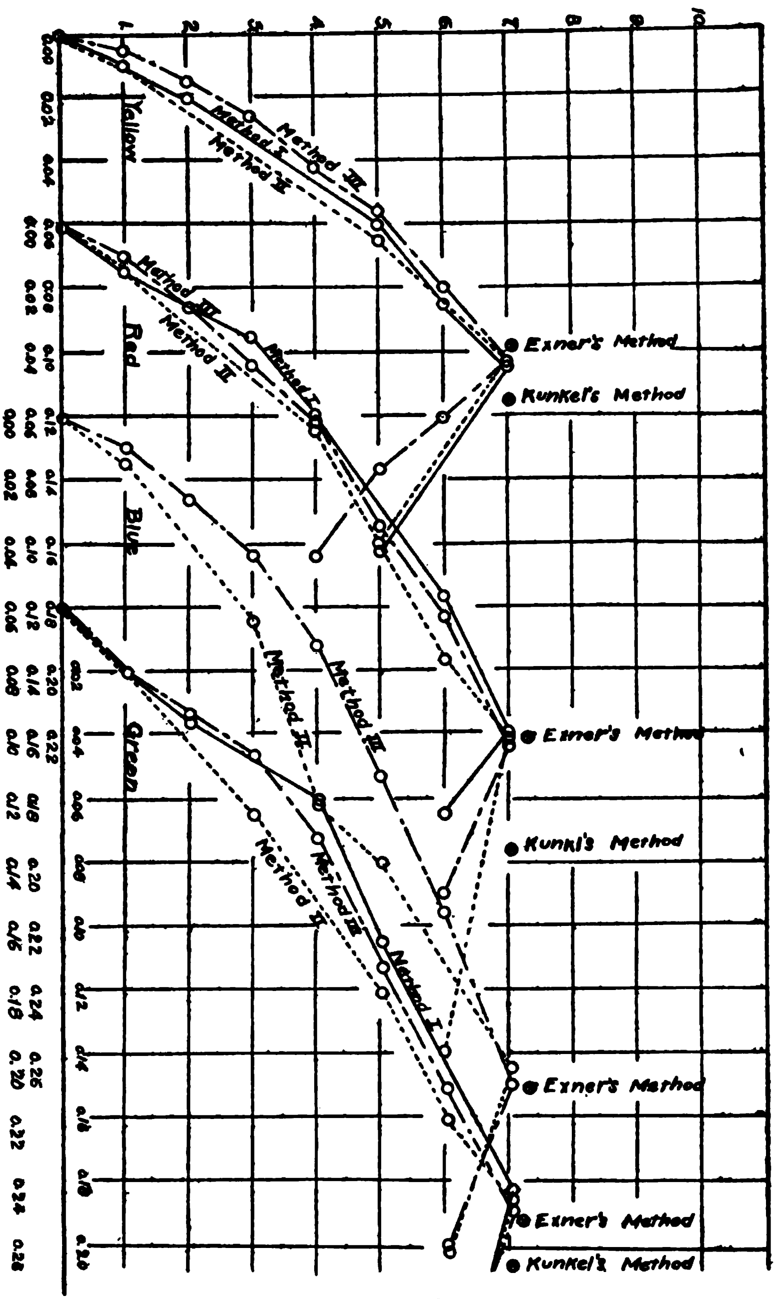


FIGURE XX
Showing a comparison of the results of a determination of the rise of sensation by the different methods used in this investigation. The lights used to produce the sensations under investigation were made photometrically equal at 0.057 meter-candles.

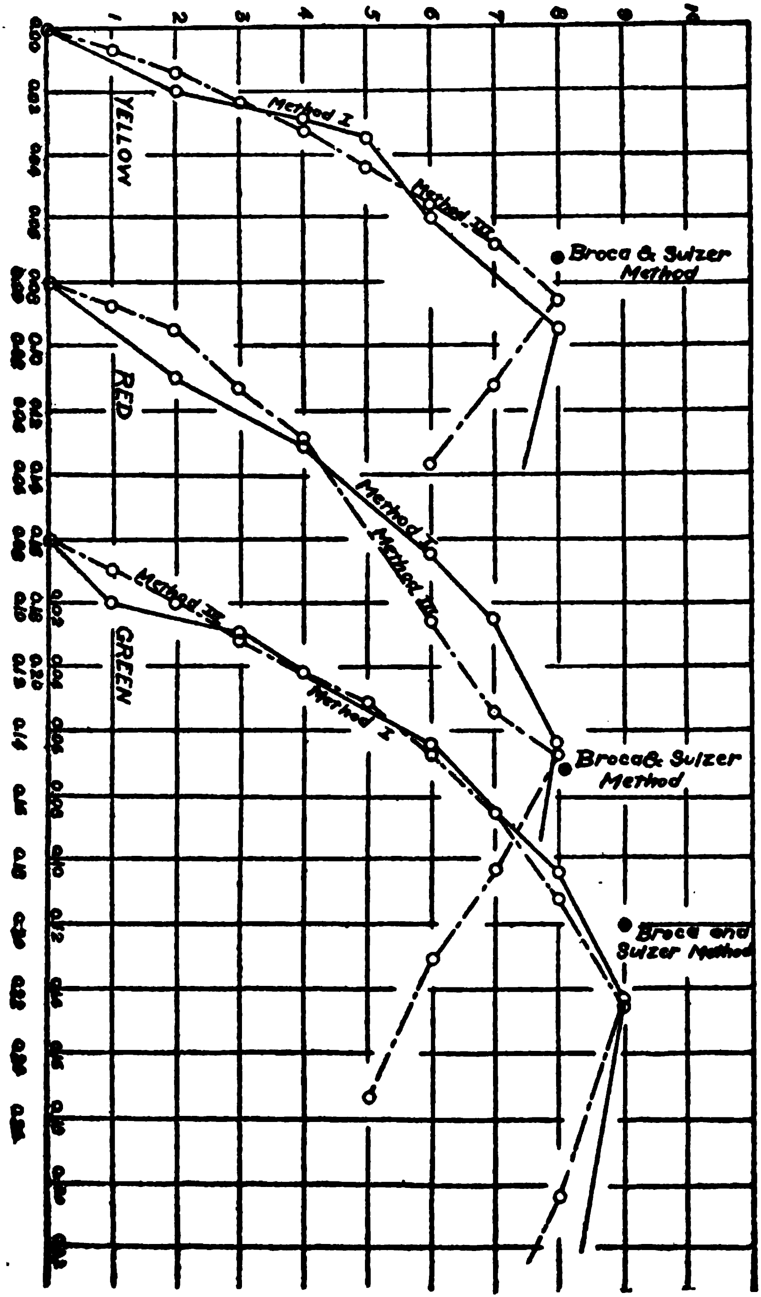


FIGURE XXI
Showing a comparison of the results of a determination of the rise of sensation by the different methods used in this investigation. The lights used to produce the sensations under investigation were made photometrically equal at 0.151 meter-candles.

the same observer and with a similar technique and apparatus. It would be a matter of a great deal of interest if we could also compare the results obtained by the originators of these methods with our own; but, as we have already stated in giving our reason for making the comparative study ourselves, this has not been possible. In reviewing all of the results which have been obtained by these investigators there seem to be but two sets which can be compared with our own. These are the results given by Büchner and by Lough on the time that is required by differen intensities of white light to arouse the maximum of sensation. In all other cases the specifications of the light with regard to composition and intensity were too fragmentary and indefinite to allow of any comparison; or, in the case of Broca and Sulzer an entirely different range of intensities was used. It is rather remarkable that in comparing these results with ours so close an agreement should be found considering that the lights were only of the same order of intensity and that we have no *a priori* reason to suppose that the lag is the same for all observers. Table XVII shows a comparison of these sets of results.

TABLE XVII

Observer	*Intensity in meter-candles*	*Time necessary to arouse maximum response*
Bills	0.057	0.216
Büchner	0.072	0.230
Lough	0.097	0.148
Büchner	0.145	0.200
Bills	0.150	0.180
Büchner	0.260	0.120
Lough	0.288	0.110
Bills	1.211	0.105
Lough	1.552	0.100
Lough	6.208	0.080
Lough	24.832	0.090

It is very probable that there is a closer agreement from observer to observer in the amount of lag for white than for colored light. The author hopes as one of the features of a later work to make a comparative study of this phenomenon for both colored and white light with a number of observers using the same method of making the determination and the same intensity of light.

CONCLUSION

In conclusion, the following points may be mentioned: (1) Previous investigators of the lag in the responses of the retina to lights of different compositions and intensities have worked with one method alone and have not, as a rule, specified their conditions of experimentation with sufficient definiteness and precision that their work can be reproduced or their results be compared with each other. Moreover, only one of the methods used by them was designed to give the rate of rise of sensation to its maximum as well as the time required for the maximum to be reached; and the principles on which this method is based are open to criticism.

(2) The object of the present study has been to devise new methods and to compare the results obtained by them with those obtained by the more promising of the older methods with the same observer, under the same conditions of experimentation and with the same wave-lengths and intensities of light. In a preliminary study four of the older methods, with some needed modifications, were chosen for the purposes of this comparison.

(3) A very close agreement in the rate of rise of sensation was obtained by the new methods for each kind and intensity of light used. A close agreement also was obtained by all of the new methods, and two of the old for the time required for sensation to reach its maximum in each of the cases compared. A poorer agreement was obtained by the method of Kunkel and a still poorer one by the method of Dürr. Both of these latter methods also proved very difficult and cumbersome of application.

(4) Of the new methods, 1 and 3 seem to have the greatest *a priori* sureness of principle, also to agree the most closely with each other in result. Of these, 3 is the most feasible and convenient of application and of all the methods we have used, seems to be the most promising for general laboratory purposes.

(5) A different rate of rise of sensation is found for the dif-

ferent wave-lengths and for white light. That is, the retina is not only selective in the amount of its response to wave-length, but it is selective also in the lag it shows in giving its full response. This selectiveness in lag of response also varies with the intensity of the light used.

(6) With the highest intensity of light used, 1.211 meter candles, the order of rate of rise (Method 3) from fastest to slowest, up to within two just noticeable differences of the maximum for green, was green, yellow, white, and red. At this point green lags behind the other colors. Blue was not used in this determination because it could not be obtained of the required photometric value with our apparatus. With the lowest intensity of light used, 0.057 meter candles, the order was yellow, red, blue, green and white. With the intermediate intensity 0.151 meter candles the order varies at different stages in the rise. Near the maximum it is yellow, green, blue, white, and red. Near the threshold it is red, yellow, blue, green, and white. Farther up, near the middle of the course (at the third just noticeable difference), it is blue, yellow, green, red, and white.

(7) With increase of intensity of light there was a decrease in the time required to produce the maximum response. This decrease was more rapid for green and blue and slower for red and yellow.

(8) It was stated in our introductory section that one of the objects of this comparative study of methods was to find one that would be sufficiently feasible in operation to be used in an investigation of the characteristic difference in the results obtained by different observers with the equality of brightness and flicker photometers. Method 3 is recommended for this purpose. In this investigation determinations of lag and photometric determinations should be made by the same observers for the same kinds and intensities of light with the object of finding out whether the deviations of the photometric balance obtained by the method of flicker from that obtained by the equality of brightness method are, for a given observer, in accord with what might be expected from the amount and distribution of lag for that observer. An investigation of this kind is already in progress in the Bryn Mawr laboratory.

Vol. XXVIII
No. 6
PSYCHOLOGICAL REVIEW PUBLICATIONS
Whole No. 128
1920

THE
Psychological Monographs

EDITED BY

JAMES ROWLAND ANGELL, 522 Fifth Avenue, New York.
HOWARD C. WARREN, Princeton University (*Review*)
JOHN B. WATSON, New York (*J. of Exp. Psych.*)
SHEPHERD I. FRANZ, Govt. Hosp. for Insane (*Bulletin*) and
MADISON BENTLEY, University of Illinois (*Index*)

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO

Some Factors Determining the Degree of Retroactive Inhibition

BY
EDWARD STEVENS ROBINSON, Ph.D.
Assistant Professor of Psychology, The University of Chicago

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
and LANCASTER, PA.

Agents: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W. C.)
Paris (16 rue de Condé)

Psychological Review Publications

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, NEW YORK (*J. of Exp. Psych.*)
JAMES R. ANGELL, 522 FIFTH AVENUE, NEW YORK (*Monographs*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*)
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

PSYCHOLOGICAL BULLETIN

containing abstracts, critical reviews, notices of books and articles, psychological news and notes, university notices, and announcements, appears monthly, the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 480 pages.

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in May, and may be subscribed for in connection with the periodicals above, or purchased separately.

THE PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. THE MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.

ANNUAL SUBSCRIPTION RATES

Review: $4.25 (Foreign, $4.50). **Review and Bulletin:** $9.00 (Foreign, $9.50).
Journal: $4.25 (Foreign, $4.50). **Review and Journal:** $8.25 (Foreign, $8.75).
Bulletin: $5.00 (Foreign, $5.25). **Journal and Bulletin:** $9.00 (Foreign, $9.50).
Any one of above with **Index:** $1.25 additional.
Any two of above with **Index:** $1.00 additional.
Review, Bulletin, and Journal: $12.50 (Foreign, $13.25).
Review Bulletin, Journal, and Index: $13.50 (Foreign, $14.25).
Current Numbers: Review or Journal, 80c.; **Bulletin,** 50c.; **Index,** $1.50.
Psychological Monographs: $5.50 per volume (Foreign, $5.80).
Current Issues: prices vary according to size.

Philosophical Monographs: a series of treatises more philosophical in character.
Library of Genetic Science and Philosophy: a series of bound volumes.

Subscriptions, orders, and business communications may be sent direct to the

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, NEW JERSEY
AND LANCASTER, PA.

FOREIGN AGENTS: G. E. Stechert & Co., London (2 Star Yard, Cary St., W. C.); Paris (16, rue de Condé)

CHAPTER I

HISTORICAL INTRODUCTION

This study is concerned with the fact that the interpolation of certain forms of activity between learning and recall may positively accelerate the forgetting process. The general phenomenon is clearly demonstrated in the *retroactive inhibition* of Müller and Pilzecker[1] and in the *retrograde amnesia* of the psycho-pathologists. Taken together the several investigations of this problem that have been made during the past twenty years prove conclusively that a positive acceleration of the forgetting process does occur under certain conditions of interpolation. Just what those conditions are, however, still remains doubtful. Müller and Pilzecker, who were the first experimentalists to study this matter directly, made a number of generalizations which later work has failed to verify with anything like completeness. In light of this it has been my purpose to re-examine the facts of the case with fresh methods in order to extend or qualify previous statements.

THE EFFECT OF SIMILARITY BETWEEN ORIGINAL LEARNING AND THE INTERPOLATED ACTIVITY

Müller and Pilzecker made the statement that retroactive inhibition, as they obtained it, is not due to the similarity of the original learning to the interpolated activity. They worked with the conventional nonsense syllable material. After the study of a given series of syllables, a definite time interval was allowed to elapse before the test for the series was made. This interval was given over to "rest" or to some form of mental activity, usually the study of a second series of syllables. Both the "Right Associates" and "Saving" methods were employed. In the former case recall time was measured by means of a Hipp chronoscope and sound-key. Retention after work is compared with retention after rest in eight slightly differing instances. The results show that recall of a previously learned list of syllables is not as complete nor as rapid after a period of prescribed mental activity as it is after a period of rest, *i. e.,* a period during which activity is free except for the fact that the original syllables must be kept out of mind. In all but two of the experiments of Müller and Pilzecker interpolated work consisted in the study of a series of syllables. The two exceptions, however, are crucial in the minds of these investigators. In both cases the subjects studied pictures instead of syllables during the interpolated period. The process was memorial in that the subjects were called upon to describe the pictures after they were removed from view. The results here show that recall is not so good after studying the pictures as it is after an equal period of so-called rest. Moreover, a comparison of these results with those where syllables were studied during the interpolated interval shows that the forgetting process is as much accelerated in one case as it is in the other. Upon this fact Müller and Pilzecker based their conclusion that there is no relationship between similarity of interpolated activity to original learning and the amount of retroactive inhibition.

---

[1] **Müller, G. E., and Pilzecker, A., Zeitschrift für Psychologie, Ergänzungsband I, 1900.**

Fräulein Heine,[2] with methods very much like those of Müller and Pilzecker, obtained results which in the main substantiate those of the earlier investigators. Nineteen experiments in recognition memory showed no evidence of retroactive inhibition. Positive results were obtained, however, from a series of experiments in recall memory. The "Right Associates" and "Saving" methods were employed. For interpolated work the subjects studied pictures, lists of four-place numbers, or consonants. The main memorized material was again nonsense syllables. All forms of interpolation show inhibition as compared with rest. Unfortunately Heine's experiments were so arranged that no accurate estimate can be made of possible differences in effectiveness between the forms of interpolated work which she introduced. Unfortunately, too, her results give no basis for comparing the effectiveness of interpolated work similar to the original learning with that of interpolated work comparatively dissimilar to the original learning. Probably she accepted the comparison made by Müller and Pilzecker as needing no further test.

---

[2] Heine, R., Zeitschrift für Psychologie, 68, 1914, pp. 161-236.

Before Heine's work was published in full, DeCamp[3] carried out a series of experiments which he hoped might give more exact definition to the conception of retroactive inhibition formulated by Müller and Pilzecker. In most of his experiments DeCamp used nonsense syllables for original learning and multiplication for interpolated work. DeCamp's results, unlike those of Heine, show little evidence of retroactive inhibition being at work. On the whole recall after rest is better than recall after work, but, considering the individual experiments, this result is not very decisive. DeCamp concludes from his own results that retroactive inhibition is of questionable importance where original learning and interpolated work are of a different nature. He suggests that the effects of retroactive inhibition may vary directly as the similarity of interpolated work to the learning. This is by way of an aftermath, however, since he has no data to support this hypothesis except in the negative sense that he obtained little inhibition where learning and interpolated work were dissimilar.

Tolman,[4] like DeCamp, was interested in further investigating retroactive inhibition where interpolated work and original learning are comparatively dissimilar. The originally memorized materials were words, numbers and nonsense syllables. For interpolated work the subjects memorized numbers or consonants. The method of "Right Associates" was used throughout. Although no attempt was made to compare the effects of similar and dissimilar interpolated work, the results throw further light upon DeCamp's suggestion that retroactive inhibition is of doubtful importance where interpolated work and original learning are

[3] DeCamp, J. E., Psychol. Monog., 1915, 19.

[4] Tolman, E. C., Psychol. Monog. 1917-1918, 25.

dissimilar. Tolman's results are more in accord with those of Müller and Pilzecker and Heine. Out of eleven experiments bearing on this point, eight give positive evidence of inhibition being present, two are ambiguous, while one shows better recall after work than after rest. Thus, under certain conditions at least, the retroactive effects of dissimilar interpolated work may be quite marked.

Webb[5] and Brockbank[6] studied the effects of interpolated activities upon the retention of motor habits. Both used mazes for the original learning. Webb, who worked on both rats and humans, employed similar interpolation, i. e., the mastery of other mazes. His results are positive. The learning of a second maze has a disintegrating effect upon a maze previously learned for all but 37% of his subjects in one experiment and for all but 26.7% in another. In Brockbank's investigation, where only rats were used, the interpolation was comparatively dissimilar to the original learning. After acquiring a maze habit his test group began practice on a rope-ladder problem. A comparison of the retention of the original maze habit by this group with that of a group which did not work on a second problem shows a lack of disintegrating effect for the interpolated learning. In fact the rats who learned the second problem show better retention than those who did not. The positive findings of Webb and the negative findings of Brockbank (that is, negative so far as a disintegration process is concerned) suggest that at least in the field of motor learning there may be a very real relationship between the similarity of interpolated activity to original learning and the amount of retroactive inhibition.

Von Kries,[7] E. Meyer,[8] Bigham[9] and McC.Gamble[10] mention indirectly the disintegrative effects of interpolated activities, but they bring out few facts which throw any light upon the results of the major investigations in this field.

---

[5] Webb, L. W., Psychol. Monog., 1917, 24.

[6] Brockbank, T. W., Behav. Monog., 1919, 4.

[7] and [8] See Müller and Pilzecker, S. 194 and DeCamp, p. 7.

[9] Bigham, J., Psychol. Rev., 1894, 4.

[10] McC.Gamble, E. A., Psychol. Monog., 1909, 10.

It seems then that aside from the meagre results of Müller and Pilzecker and a rather questionable comparison of the results of Webb and Brockbank, we have no grounds for generalizing about the influence similarity between interpolation and learning may have on the process of forgetting. Evidently the problem demands further experimentation.

TEMPORAL POSITION OF INTERPOLATED WORK

In an experiment with a single subject Müller and Pilzecker compared the amount of retroactive inhibition where the interpolated work followed 17.2 seconds after memorizing with the amount of retroactive inhibition where the work followed 6 minutes after memorizing. Considering amount of recall, the inhibition is greater under the first condition. The average recall time shows the opposite result, but there are more very short recall times under the second condition. Müller and Pilzecker conclude from these not unequivocal results that the earlier the work is engaged in after memorizing, the greater is the resulting inhibition.

DeCamp was mainly concerned with the degrees of retroactive inhibition for different amounts and distributions of interpolated work. His results fail to verify the Müller and Pilzecker generalization regarding the effect of variation in the temporal position of interpolated work. Quite possibly, however, this is due to the fact that under few of DeCamp's conditions was retroactive inhibition clearly present.

INFLUENCE OF DEGREE OF LEARNING

Heine carried out seven experiments in order to discover the relationship between degree of learning and susceptibility to retroactive inhibition. Two of these employed the "Saving" method and five the method of "Right Associates." Each experiment was a comparison of the amount of inhibition present where original lists of nonsense syllables were given more or fewer repetitions. Each experiment used one subject and two degrees of learning. Her results in all cases show greater inhibition for the lists given the fewer repetitions. This fact suggests that the amount of inhibition is inversely related to the degree to which the disintegrated material is learned.

INFLUENCE OF CONDITIONS OF LEARNING

Tolman studied the relationship between conditions of learning and susceptibility to retroactive inhibition. He measured this susceptibility under the following four pairs of conditions:

(1) Pleasant vs. Indifferent Materials
(2) Normal vs. Distracted Attention
(3) With Caffeine vs. Without Caffeine
(4) Efficient vs. Inefficient Working Periods

(1) The memorized material in this series of experiments consisted of word lists or lists made up of numbers and words. These lists, by reference to the words they contained, were pleasant or indifferent according to the judgments of a number of observers. Four experiments were carried out. In the first of these retroactive inhibition is plainly evident and it is greater for the indifferent lists. The next three experiments show little evidence for the presence of inhibition, but what evidence there is favors greater inhibition for the indifferent lists.

(2) In the single experiment under this head the subject learned lists of alternate nonsense syllables and words. As each pair of list elements was shown, a telegraph key clicked once, twice, or three times and the subject duplicated the taps by striking on the table with a pencil. Less inhibition is shown for lists learned under distracted attention, but the inhibition is so small in both cases that no certain conclusion can be drawn.

(3) In these experiments the subjects took one and one-half grain capsules of caffeine on alternate days and sugar of milk capsules on the days between. They were unaware of which were caffeine and which non-caffeine days. Of the two experiments conducted, both give some evidence for greater inhibition on the non-caffeine days.

(4) There were six experiments in the last group. Lists of nonsense syllables learned in a morning period were compared with lists learned directly after the midday meal or in the eve-

ning. The results are pretty clearly in favor of greater inhibition for the afternoon and evening lists.

It is evident from Tolman's work that susceptibility to retroactive inhibition is affected by the conditions of learning. Under what he calls more stimulating conditions of learning—learning pleasant material, learning after taking caffeine, learning in an efficient hour of the day—there is less probability of retroactive inhibition being effective than under less stimulating conditions—with indifferent material, without caffeine or in an inefficient hour of the day.

Relative Susceptibility of Men and Rats to Retroactive Inhibition

In Webb's work human subjects show greater susceptibility to retroactive effects than do rats. Under six conditions where comparison is possible a larger percentage of human subjects manifested some degree of what Webb calls negative retroaction. In three groups of human subjects 100% of the individuals were affected, while in no instance were all the rats of a group affected. In terms of absolute and relative loss the average disturbance for the human groups is clearly greater than for the rat groups.

Direction of Acquiring Two Habits and Retroactive Inhibition

Webb's experiments were so arranged that he could determine whether the retroactive effects of two activities upon each other were the same or different. For instance, it was possible to compare the effect of learning maze B after maze A upon the relearning of A with the effect of learning maze A after B upon the relearning of B. Various pairs of conditions or sequences, as A — B and B — A, were ranked according to the amount of retroaction present. While the correlations between these rankings are not perfect, Webb concludes that they are high enough to indicate that direction of learning is not a deciding factor in determining the amount of retroactive inhibition.

INFLUENCE OF THE DIFFICULTY OF INTERPOLATED ACTIVITY

encoding

Webb found that the easier an interpolated maze is mastered the greater is the resulting negative retroaction. His various mazes were ranked in their order of difficulty. They were also ranked according to their retroactive effects. Correlation coefficients were calculated between the two sets of values. Five out of six coefficients are negative. Three of these are — 1.00 and the other two are between — 1.00 and — .60.

TRANSFER AND RETROACTION

According to Webb's findings there is a negative correlation between positive transfer and negative retroaction. The mazes used in the retroaction experiments had previously been used in transfer experiments. These mazes could be ranked, therefore, according to both the positive transfer and the negative retroaction shown by them. Twenty-four pairs of such variables give twenty-one negative correlations. Five of these are —1.00, seven are between —1.00 and —.70, and the remainder are around —.50. Webb concluded from this that the greater the assistance rendered by a first maze in the mastery of a second maze, the less disturbance there will be in relearning the first maze. The same relationship is apparent in comparing humans and rats in transfer and retroaction. The human subjects show greater susceptibility to negative retroaction and also less susceptibility to positive transfer.

PATHOLOGICAL CONDITIONS OF RETROACTION

The acceleration of the forgetting process due to organic brain diseases, epileptic attacks, hysterical attacks, convulsions, severe blows on the head, or emotional shocks is the topic of a familiar chapter in abnormal psychology. *Retrograde Amnesia* is the term usually employed to designate this group of phenomena. The literature of the subject abounds in concrete illustrations. For example: "While in an intoxicated condition a man received a blow on the head while resisting arrest. Following the injury he was unconscious for eight or ten hours, and on regaining consciousness found that he was unable to recall any events of the week previous. The memories of the amnesic period have never spontaneously returned, although a period of several years has elapsed. His only knowledge of the events of the week comes through information gathered from friends."[11] Similar illustrations might be cited of the retroactive effects of other disturbing conditions.

In this connection it might be well to mention that cases of Retroactive Hypermnesia have also been observed.[12] In such cases commonplace and relatively insignificant events taking place before some shock or emotional crisis are afterwards recalled with unusual vividness.

THEORIES OF RETROACTIVE INHIBITION[13]

(1) *The Perseveration Theory.* Müller and Pilzecker believed that the processes underlying perseveration and retroactive inhibition are the same. According to this theory memorizing is followed by a period of gradually diminishing activity of the neural elements involved in the memorizing. Evidently this after-activity is of vital importance in the setting-in of the memory pattern. Now, the introduction of any kind of strenuous mental work while this setting-in is still going on will interfere in some way with the progress of the setting-in process. Memorized material, therefore, will not be as well retained where memorizing is followed by mental work as where it is followed by comparative rest. As the after-activity diminishes gradually, the deleterious effects of interpolated work will vary inversely as the time elapsing between the end of memorizing and the beginning of the work. This theory is supported by two of the findings of Müller and Pilzecker: (1) One kind of strenuous mental work gives as great inhibitory effects as another, although they differ decidedly in their relative resemblance to the original

---

[11] Coriat, I. H., Abnormal Psychology, 1910, p. 183.

[12] Stratton, G. M., Psychol. Rev. 1919, 26.

[13] In presenting these theories I have not in all cases attempted to use the exact language of the writers referred to.

memorizing; and (2) Interpolated work beginning 17.2 seconds after memorizing shows greater inhibitory effects than interpolated work beginning 6 minutes after memorizing. This explanation of retroactive inhibition has received rather wide acceptance, but it should be remembered that the experiments from which Müller and Pilzecker derived their most critical results are not very convincing from a quantitative point of view. Furthermore, later work, while it has disproved none of their main tenets, has yet failed to verify them.

(2) *The Transfer Theory.* DeCamp suggested that the small amounts of inhibition which he obtained might be explained by an extensively modified form of a theory commonly used to explain transfer of training. "From the neurological standpoint, in the learning of a series of syllables, we may assume that a certain group of synapses, nerve cells, nerve paths, centres, etc. are involved. Immediately after the learning process the after-discharge continues for a short time, tending to set the associations between the just learned syllables. Any mental activity engaged in during this after-discharge, involving or partially involving the same neurological group, tends, more or less, to block the after-discharge and to give rise to retroactive inhibition. Engagement in any mental activity involving a new—so far as it is new—group of synapses, neurones, etc., would allow the setting-in process of the just excited group to proceed unhindered. The effect of retroactive inhibition would vary directly as the relative identity of the neurological groups involved."[14] This theory can not be taken literally, of course, because, in progressing toward greater and greater identity of neurological groups, we must reach a point where the good effects due to repetition are greater than the bad effects due to interference. It may hold, however, within certain limits.

DeCamp's theory differs from that of Müller and Pilzecker in that the inhibition is supposed by him to be a function of the relative identity of original memorizing and interpolated work, rather than of the difficulty of the interpolated work. The two theories are alike in that both consider the interruption of a setting-in process as the primary cause of the disintegration or inhibition.

---

[14] Op. Cit. p. 62.

(3) *Other Explanations*. Webb has suggested two explanations of retroactive inhibition in terms of transfer without reference to a possible setting-in process following memorizing. In the first place, certain elements in the original learning process may become transferred from the pattern of the original learning to the pattern of the interpolation, so that the recall of the originally learned material is interfered with. In the second place, there may be a transfer of elements of the interpolation 2 to the situation in which the original material is recalled. This introduction into the recall of certain extraneous elements may have deleterious effects. Webb points out that there is no theoretical reason why these effects should always be deleterious; they may conceivably be facilitative as well as inhibitory.

There are still other explanations which may be applied to special cases of accelerated forgetting. Where the interpolated activity is very strenuous, the general efficiency of the organism may be so low that the process of recall can not function normally. Where, as in many cases of retrograde amnesia, the interpolation supposedly causing disintegration is distinctly pathological, the impressionability of the organism may be subnormal for some time before the evident onset of the pathological state.

EVALUATION OF CURRENT THEORIES

A consideration of current theories of retroactive inhibition, or whatever one chooses to call it, in light of our empirical knowledge indicates that none of these theories has any great amount of factual support. Further observations are necessary before we can accept any one of them either as it stands or in modified form. It is quite possible, too, that no single theory may ever suffice and that radically different explanations may have to be formulated for the different conditions under which the forgetting process is positively accelerated.

The Most Immediate Problems

The subject of retroactive inhibition offers almost limitless opportunities for investigation. But two problems, it seems to me, especially deserve attention: (1) The relationship between retroactive inhibition where learning and interpolation are similar, and retroactive inhibition where the two are comparatively dissimilar; and (2) The influence of the temporal position of interpolated work upon the degree of retroactive inhibition. Experiments on both of these topics are included in the present study. There is also a third investigation which deals with the susceptibility to retroactive effects of materials learned with varying degrees of completeness.

CHAPTER II

EXPERIMENTS WITH INTERPOLATION SIMILAR AND DISSIMILAR TO ORIGINAL LEARNING

Three experiments were conducted to determine whether the degree of retroactive inhibition is in any way related to the similarity between the interpolation and original learning. This question, as I have pointed out, has already received both an affirmative and a negative answer. But the affirmative answer given by DeCamp has no basis in experimental fact, and Müller and Pilzecker's negative answer is based upon very limited data.

EXPERIMENT I

MATERIALS, METHODS, AND SUBJECTS

The originally memorized material consisted of series of eight four-place numbers. These series were made up by chance, with the following limitations:

(1) No digit within one number occurs in the immediately preceding or following number.

(2) No digit occurs twice in the same number.

(3) No digit immediately precedes another digit one more in value.

(4) The numbers 1357, 2468 and 3579 were never used.

Each number series was typed on a three-by-five-inch card. The presentation was simultaneous and the subject was given the following instructions:

"I have here a series of four-place numbers. You will have three minutes during which to study them. Later I shall ask you to reproduce in proper order as many of these numbers as you can."

Three minutes after presenting the number series the experimenter removed it from before the subject and directed him to engage in one of five forms of interpolated activity for a period of three minutes. An average of approximately 15 seconds elapsed between the end of the three minutes of original memorizing and the beginning of the three minutes of interpolated activity. The interpolated activities and the directions the subject received for each were as follows:

(A) *Numbers*—Memorizing a second series of four-place numbers.

Directions: "I have here another series of four-place numbers. You will have three minutes during which to study them. Later I shall ask you to reproduce in proper order as many of these numbers as you can."

(B) *Consonants*—Memorizing a series of twenty consonants arranged in a vertical column. Each series was a chance arrangement of all the consonants except w. No consonant, however, immediately followed or preceded a consonant adjacent to it in the alphabet.

Directions: "I have here a series of consonants. You will have three minutes during which to study them. Later I shall ask you to reproduce in proper order as many of these consonants as you can."

(C) *Poetry*—Memorizing a passage of descriptive poetry. Each of the three passages used was selected from Sir Walter Scott's Rokeby. They were the first 18 lines of Stanza XVII, Canto Second, the first 18 lines of Canto Fifth, and the first 20 lines of Canto Sixth.

Directions: "I have here a passage of poetry. You will have three minutes during which to study it. Later I shall ask you to reproduce as much of this poetry as you can."

(D) *Multiplication*—Solving four-place by four-place multiplication problems. Three sets of twelve problems each were used. Each problem was a chance arrangement of the eight digits from 2 to 9 inclusive. Each set was typed on a sheet of paper which allowed ample space for the subject's calculations.

Directions: "I have here a series of multiplication problems. You will have three minutes during which to work on them. Work as accurately and as rapidly as you can."

(E) *Reading*—Reading simple narrative prose with no attempt to memorize. The following three stories by Richard Harding Davis were used: Miss Delmar's Understudy, The Editor's Story, and The Man with One Talent.

Directions: "I have here a story. You will have three minutes during which to read in it. Read naturally and make no attempt to memorize what you read." The subject began his reading at the beginning of the story with which he was presented.

In all cases the subjects were unable to finish with the material upon which they were working during the three-minute interpolated interval.

Immediately after the three-minute interpolated period, the experimenter placed pencil and paper before the subject and said: "Write here in proper order as many as you can of the series of numbers (or "first series of numbers" in case the interpolated activity was also memorizing a number series) which you studied. Work as accurately and rapidly as you can." These directions took about 8 seconds. By means of a stop-watch the experimenter measured the time between the end of his directions and the subject's signal that he was through with the reproduction.

As soon as the subject had completed his work on the reproduction of the original number series he was asked for a reproduction of the interpolated material where, as in (A), (B), and (C), it was of a sort to be memorized.

The plan of the experiment can be seen as a whole in the following outline:

Step (1) Directions for original memorizing.
Step (2) Memorizing numbers—3 minutes.
Step (3) Directions for interpolated activity—approximately 15 seconds.
Step (4) Memorizing numbers,
Memorizing consonants,
Memorizing poetry, —3 minutes
Multiplying, or
Reading.
Step (5) Directions for recall of the original material—approximately 8 seconds.

Step (6) Recall of original number series.
Step (7) Directions for recall of interpolated material.
Step (8) Recall of interpolated material.

Four-place numbers were chosen for original memorizing because of the ease with which lists of fairly constant difficulty could be prepared, and because pairs of such lists selected at random seemed more apt to have, between the members of each pair, a constant degree of similarity than pairs of lists of, say, nonsense syllables.

The interpolated activities employed were selected as fairly representative of activities which may operate retroactively in similar real-life situations. Memorizing numbers was selected because of its close similarity to the original memorizing. Memorizing consonants was selected as like the original memorizing in that it is comparatively meaningless, although the two are unlike in content. Memorizing poetry is like the original memorizing in that it, too, is memorial, but it is much more meaningful. Multiplication is non-memorial, but it is similar to the original memorizing in content, *i. e.,* numbers are manipulated in both. Reading simple prose was selected as approximating the *passive* condition which other investigators have taken as *rest*. It seemed unwise to employ day-dreaming or undirected thought for two reasons: (1) Most of my subjects were inexperienced, and (2) Even with experienced subjects some objective control of *rest* was desirable.

All of the subjects stated that they never rehearsed the original number series during the interpolated interval. Now and then a subject would report that one or two of the numbers had come to mind, involuntarily as it were, during the interpolation.

Fifteen subjects served in the present experiment. There were eight women and seven men. Five men and five women were undergraduates in the University of Cincinnati, one man was an undergraduate in the University of Chicago, and three women and one man were graduate students of psychology in the University of Chicago. None of the subjects was aware in any detailed way of the purpose of the experiment.

Each of the fifteen subjects went through each of the five conditions of the experiment three times. The regular experimentation was preceded by five sittings at which the subjects obtained practice in studying and reproducing series of four-place numbers. During this practice the study period was three minutes in length, and recall came immediately after the completion of the study period. Only one series of numbers was studied at each of the practice sittings. Before the completion of the practice most of the subjects formulated a study method which they followed throughout the experiment.

A further precaution was taken to guard against effects which practice might have on the final results. The subjects were divided into three groups of five each. Each subject within a given group went through the five conditions in different order, so that each condition was favored by equal amounts of practice in the general procedure of the experiment. Table I shows the arrangement of experimental conditions for a single group during a single five-day cycle. Subsequent cycles to the first were repetitions of the same arrangement.

TABLE I

Subject		Day 1	Day 2	Day 3	Day 4	Day 5
1	Cond.	A	B	C	D	E
2	Cond.	B	C	D	E	A
3	Cond.	C	D	E	A	B
4	Cond.	D	E	A	B	C
5	Cond.	E	A	B	C	D

Experimentation, including preliminary practice, was conducted on five days (Monday to Friday) of four weeks. In the case of the first two groups the Thanksgiving recess made it necessary to suspend work during the fourth successive week. It was resumed and concluded, therefore, during the fifth week. The same conditions were adhered to in work with the third group, which was carried on a year later than the work with the other two.

On any given experiment day all subjects had the same number series for their original memorizing. This fact at least partially counteracted possible inequalities in the difficulty of different series, for each interpolated activity was tested once with each series.

It was impossible to test all subjects at the same time of day, but, within any week, the sitting times for the individual subjects were kept practically constant. In three or four instances it was necessary, because of unpredictable circumstances, to omit a day's work during the regular week and add on a Saturday sitting, or to work with some individual from Tuesday until Saturday instead of from Monday until Friday.

The subjects were directed not to talk about the experiment to anyone until it was completed.

CRITERIA OF RECALL

In order to measure the degrees of retroactive inhibition present under the different conditions of the experiment, it was necessary to compare the efficiency of recalling the original numbers after the five kinds of interpolation. Three criteria of recall were selected: (1) Amount of Recall—the number of digits recalled irrespective of their correctness; (2) Error of Recall—the percentage of the total digits recalled which were incorrect or in incorrect position. Each four-place number which was one place removed from its correct position was counted as a single error. A four-place number more than one place removed from its correct position was counted as four errors. (3) Time of Recall—the total time taken for reproduction divided by the total number of digits given in the reproduction.

These criteria were employed in all of the experiments of this research except Experiment III.

RESULTS

The contents of Table II were obtained by averaging the records of the fifteen subjects. Each subject's record was determined by taking the average of his performance under each condition in the three cycles or weeks of the experiment.

By all criteria the interpolation of a second series of numbers has the greatest disintegrating effect upon a number series previously learned. Reading causes the least inhibition by the

TABLE II

CONDITION	AMOUNT	ERROR	TIME
Numbers	15.4 digits	27.5%	5.73 Secs.
Consonants	22.4 "	17.4%	3.07 "
Poetry	21.9 "	19.3%	3.57 "
Multiplication	22.1 "	15.7%	3.55 "
Reading	22.8 "	16.5%	3.03 "

amount and time criteria and next to least by errors. Poetry causes the next to most inhibition by all three criteria. Between consonants and multiplication there is little basis for differentiation. These two seem to lie somewhere between Poetry and Reading in the degree of inhibition shown. According to this table, then, Numbers shows the most inhibition, Poetry the next most, Consonants and Multiplication the next most, and Reading the least.

The conditions other than Numbers, however, differ from each other by amounts whose statistical certainty is questionable. Each average represents, in a sense, 45 individual measures and upon this basis it might seem profitable to evaluate these averages by the ordinary probable error method. On the other hand, the 45 measures are distributed among only 15 subjects. In other words the actual number of cases from which each average is figured is 15, too small a number about which to make any such assumptions as are required by a method reliably applicable only to normally distributed variables. It will be necessary, therefore, in further estimating the results of this experiment, to inspect the records of the individual subjects in some detail.

Tables III, IV, and V present for the three criteria the average performance of each of the fifteen subjects under each condition of interpolation. There are also entered in these tables the ranks of the different conditions according to the amounts of inhibition which they show. Where, under *Numbers*, a figure *1* appears in the column headed *Ranks*, it means that for the subject concerned recall was most disturbed after the interpolation of numbers. A rank of *5* means that a given subject, under a given condition, was disturbed least.

These tables will be examined with two main questions in mind: (1) Does the interpolation of numbers clearly show more inhibition than the other conditions? and (2) Are there any convincing differences among those other conditions?

TABLE III

Amount of Recall

(Total Digits)

	Numbers		Consonants		Poetry		Multiplication		Reading	
SUBJECT	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS
1	18.7	4	17.3	2	18.7	4	16.7	1	18.7	4
2	8.3	1	17.7	5	14.7	4	13.3	3	13.0	2
3	14.7	5	11.7	4	9.3	2	7.3	1	10.7	3
4	23.0	1	23.3	2	25.3	3	30.0	5	27.3	4
5	23.0	3	22.7	1.5	22.7	1.5	24.3	4	24.7	5
6	16.3	1	27.3	4	21.0	2	24.3	3	29.0	5
7	5.3	1	28.0	5	25.3	3.5	22.7	2	25.3	3.5
8	8.0	1	22.7	3	21.0	2	24.7	5	24.0	4
9	12.0	1	24.0	3	25.3	4.5	25.3	4.5	23.0	2
10	30.0	2	30.0	2	30.7	4	30.0	2	32.0	5
11	16.0	1	32.0	4	32.0	4	30.7	2	32.0	4
12	12.0	4	8.3	2	12.3	5	9.0	3	6.0	1
13	21.7	5	18.3	1	19.3	3	18.7	2	20.7	4
14	14.3	1	32.0	4	24.7	2	32.0	4	32.0	4
15	8.0	1	20.7	2	26.0	5	22.3	3	23.7	4

TABLE IV

Error of Recall

(% of Recall Erroneous)

	Numbers		Consonants		Poetry		Multiplication		Reading	
SUBJECT	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS
1	28	4	30	2.5	40	1	24	5	30	2.5
2	20	3	22	2	23	1	4	4	0	5
3	17	2	9	4	49	1	8	5	15	3
4	32	1	23	3	19	4	24	2	18	5
5	25	3	20	4	16	5	29	1.5	29	1.5
6	48	1	18	5	33	3	34	2	21	4
7	25	1	3	5	23	2	6	4	7	3
8	19	2	18	3	22	1	16	4	4	5
9	11	1.5	3	4	0	5	6	3	11	1.5
10	54	1	39	2	28	3	20	5	27	4
11	10	1	0	4.5	4	3	0	4.5	9	2
12	25	1	23	2.5	11	5	23	2.5	20	4
13	35	1	32	3	7	5	13	4	33	2
14	31	1	3	5	4	3.5	14	2	4	3.5
15	33	1	18	3	11	5	15	4	20	2

TABLE V

TIME OF RECALL

(*Av. Time in Secs. per Digit*)

	Numbers		Consonants		Poetry		Multiplication		Reading	
SUBJECT	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS
1	7.6	2	6.7	3	6.4	5	6.5	4	7.7	1
2	7.1	1	2.0	4	2.5	3	3.1	2	1.8	5
3	1.9	5	2.8	3	4.9	2	5.6	1	2.7	4
4	4.4	1	2.6	5	3.0	2	2.8	3.5	2.8	3.5
5	4.8	1	2.4	5	4.5	2	3.4	4	4.3	3
6	4.1	1	3.0	3	3.3	2	2.6	4	1.9	5
7	6.1	1	1.9	5	2.4	4	4.5	2	3.1	3
8	13.5	1	3.8	3	4.4	2	2.3	5	2.4	4
9	2.9	1	0.9	5	1.2	3	1.1	4	1.5	2
10	7.5	2	6.5	3	6.4	4	8.4	1	4.2	5
11	5.5	1	1.4	4	1.2	5	1.6	3	2.1	2
12	1.0	5	1.8	3	1.3	4	4.4	2	5.1	1
13	3.2	3	3.4	2	4.1	1	2.3	4	2.2	5
14	12.5	1	3.3	3	6.5	2	2.8	4	2.1	5
15	3.8	1	3.5	2	1.4	5	1.8	3	1.6	4

Considering Table III, it appears that nine out of fifteen subjects show the highest degrees of inhibition under the Numbers condition. One subject shows the greatest inhibition under Consonants, two under Multiplication, and one under Reading. With one subject there is a tie for greatest inhibition between Consonants and Poetry and with another a tie between Numbers, Consonants and Multiplication. The group as a whole, then, suffers the greatest inhibition after memorizing a second number series. Among those subjects who are not most affected by Numbers, the greatest inhibition is caused by now one and now another of the other conditions, with no quantitative emphasis upon any one condition in particular.

The lack of differentiation among the conditions other than Numbers is also seen upon considering the tendency of each of those conditions to cause least inhibition—to rank fifth. Reading shows least inhibition with three subjects, and Numbers, Consonants, Poetry and Multiplication with two each. The remaining subjects show ties for least inhibition among various conditions. These differences are no greater than might be expected by chance.

Table IV shows nine subjects in whom retroactive inhibition is greatest under the Numbers condition and a tenth is most affected by Numbers and Reading, the two being tied. With four subjects there is greatest inhibition under Poetry, and with one a tie for greatest between Multiplication and Reading. This would seem to indicate that Poetry stands somewhere between Numbers and the other conditions in the amount of inhibition which it causes. Such a conclusion is contradicted, however, by the fact that in this same table five subjects have least inhibition under the poetry condition, while only three subjects show the least inhibition in each of the conditions, Consonants, Multiplication and Reading.

From Tables III and IV, then, one can safely conclude only this much; that memorizing numbers causes a greater degree of retroactive inhibition than the other conditions, and that among those other conditions there are no significant differences.

Table V tells about the same story. Ten subjects suffer most in their recall after the interpolation of numbers. Of the other five subjects, two suffer most after Multiplication, two after Reading and one after Poetry, a result which shows no decided preference for any one condition. Reading is the best condition for five subjects, Consonants for four, Poetry for three, Numbers for two, and Multiplication for one. There is some evidence here that Reading is more favored than certain of the other conditions.

SUMMARY

By the criteria of amount, errors and time, memorizing a number series between the memorizing and recall of another number series causes marked disintegration of the first series. Memorizing consonants or poetry, or solving multiplication problems have approximately equal effects upon the retention of a number series. There is a slight chance that Reading causes interpolated period, as under the Multiplication condition, but this fact is not statistically certain. In other words, the experiment indicates retroactive inhibition where original learning and interpolation are highly similar, and little or none where they are comparatively dissimilar. It is interesting to note that similar material (four-place numbers) may be dealt with during the interpolated period, as under Multiplication condition, but this fact alone does not insure any more retroactive inhibition than is obtained in certain cases where the interpolated material is quite dissimilar to the original memorizing.

EXPERIMENT II

MATERIALS, METHODS, AND SUBJECTS

The originally memorized material again consisted of series of four-place numbers. Series employed in Experiment I were also used in this experiment, since no subject served in both experiments. The method of presenting this material, the directions given the subject, and the length of time devoted to memorizing and to the interpolated activities (3 minutes for each) were the same as in the previous experiment.

Experiment II differs from Experiment I mainly in the interpolated activities used. The five interpolated activities and the directions given for each were as follows:

(A) *Four-Place Numbers*—This condition and the directions for it were the same as (A), Experiment I.

(B) *Digits*—Memorizing a series of 32 digits, the same number as contained in Condition (A), arranged in a single vertical column.

Directions: "I have here a series of digits. You will have three minutes during which to study them. Later I shall ask you to reproduce, in proper order, as many of these digits as you can."

(C) *Mental Multiplication*—Performing two-place by two-place multiplications. Each problem was made by a chance drawing of the digits from 2 to 9 inclusive.

Directions: "I am going to give you a multiplication problem. You work it in your head and give me the answer as soon as you are through. Then I shall give you another problem." This work was stopped as soon as the three minute period was up, whether the subject was in the midst of a problem or had just begun or finished one.

(D) *Pictures*—Observing attentively pictures of nudes. Two pictures were used, one for each of the two cycles of the experiment. They were colored reproductions (approximately 9 x 15 inches) of *Das Erwachen* by Anders Zorn and *Ruhende Nymphe* by Anselm Feuerbach.

Directions: "I have here a picture. You will have three minutes during which to look at it. Confine your attention to the picture during the entire three minutes."

(E) *Reading*—Reading in current editions of the *New York Times*.

Directions: "I have here a newspaper. You will have three minutes during which to read it. Read naturally and make no attempt to memorize what you read."

As in Experiment I, the interpolated period was followed by the reproduction of the original number series, and after that came the reproduction of the interpolated material, where it was of a sort to be memorized.

The interpolated activities of Experiment II were selected for various reasons. Four-place Numbers and Reading were selected for the purpose of making possible a comparison of the results of this with the results of the previous experiment. Less grounds were provided for comparing the results under Reading, of course, because the materials read differed in the two experiments, but Experiments IV and V, which had already been conducted showed that newspaper reading, like reading in short stories, causes comparatively little inhibition. The memorizing of a column of digits supplied a basis for comparing the influence of similarity of form with that of similarity of content upon retroactive inhibition. Mental Multiplication introduced a new factor into the interpolation of non-memorial numerical activity. Where the problems were done "in the head" rather than on paper, a certain sort of memorizing was required. That is, the results of completed operations had to be retained while other operations were in progress. The observation of pictures furnished an activity with some affective tone. (Most of the subjects showed some evidence of embarrassment during this interpolation, which was probably due not alone to the nature of the pictures, but also to the fact that the experimenter was a comparative stranger.)

Ten subjects served in this experiment. All were men. Nine were juniors and seniors and one a graduate student in Yale University. None was aware of the purpose of the experiment.

Each subject went through each of the five conditions twice. The experiment proper was preceded by five sittings at which the subjects obtained practice in memorizing and reproducing four-place numbers. During this practice the study period was 3 minutes in length and recall was preceded by a 3-minute period of newspaper reading.

The subjects were arranged in groups of five after the manner of Experiment I.

The criteria of recall were the same as in the previous experiment, *i. e.* (1) Amount, (2) Error, and (3) Time.

RESULTS

Table VI gives the average amount, error, and time of recall after the five different interpolations. The table was made up by averaging the individual averages for each condition. Just as in Experiment I, memorizing a second series of four-place numbers shows the most inhibition by all three criteria. Reading again shows the least inhibition by two out of three criteria and next to least by the third criterion. The actual difference, however, between Reading, Pictures, and Mental Multiplication are slight. By all three criteria, Digits is next to Four-Place Numbers in the amount of inhibition shown.

That the results of this experiment and those of the first experiment are comparable is seen in the degrees of retroaction caused by four-place numbers in the two cases. Considering Reading as a norm, the percentages of retroaction caused by Four-Place Numbers, according to the criteria of amount, error, and time, are, in the first experiment, 32, 73, and 89 respectively, and in the second 29, 77 and 63. The agreement is close, especially for amount and error.

TABLE VI

CONDITION	AMOUNT	ERROR	TIME
Four-Place Numbers	15.3 digits	33.8%	5.50 Secs.
Digits	18.2 "	22.4%	4.30 "
Multiplication	20.6 "	21.5%	4.08 "
Pictures	20.6 "	22.3%	3.15 "
Reading	21.4 "	19.1%	3.38 "

In order to estimate the reliability of these results it will be necessary, as in the previous experiment, to turn to the individual records. These are presented in Tables VII, VIII, and IX. These tables were made up in the same way as Tables III, IV, and V.

TABLE VII

Amount of Recall

(Total Digits)

	Four-Place Numbers		Digits		Multiplication		Pictures		Reading	
SUBJECT	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS
1	16.0	1	20.0	2	24.0	3.5	24.0	3.5	24.5	5
2	16.0	3.5	15.5	2	14.5	1	16.0	3.5	21.5	5
3	9.5	1	13.5	2	20.0	5	10.5	3.5	19.5	3.5
4	6.0	1	8.0	2	12.0	3	18.0	5	16.0	4
5	14.0	1	18.5	2	22.0	3.5	26.0	5	22.0	3.5
6	29.5	3.5	27.5	2	29.5	3.5	19.0	1	30.0	5
7	19.5	2.5	19.5	2.5	16.5	1	20.0	4.5	20.0	4.5
8	10.0	1	24.0	5	22.0	3.5	22.0	3.5	20.0	2
9	18.0	1.5	18.0	1.5	26.0	5	21.5	4	20.0	3
10	14.5	1	18.0	2	20.0	3.5	20.0	3.5	20.4	5

TABLE VIII

Error in Recall

(% of Recall Erroneous)

	Four-Place Numbers		Digits		Multiplication		Pictures		Reading	
SUBJECT	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS
1	56	1	24	3	28	2	18	4.5	18	4.5
2	49	5	58	3.5	58	3.5	64	2	70	1
3	58	1	28	2	0	4.5	8	3	0	4.5
4	6	5	44	1	28	3	33	2	22	4
5	28	1	3	5	16	3	25	2	13	4
6	12	3	6	4	14	2	19	1	3	5
7	40	1	31	2	19	3	15	4	10	5
8	35	1	22	3	25	2	5	5	16	4
9	28	2	0	5	5	4	31	1	19	3
10	26	1	8	4	22	2	5	5	20	3

TABLE IX

TIME OF RECALL

(*Av. Time in Secs. per Digit*)

	Four-Place Numbers		Digits		Multiplication		Pictures		Reading	
SUBJECT	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS
1	6.6	1	6.2	2	5.9	3	2.6	4	2.4	5
2	0.7	5	2.8	1	2.5	2	2.2	3	1.3	4
3	18.4	1	9.7	3	7.0	5	8.3	4	11.8	2
4	1.6	2	0.8	4.5	0.8	4.5	1.2	3	2.0	1
5	2.8	1	2.7	2	1.4	5	1.8	4	2.0	3
6	3.2	2	2.4	3	5.8	1	1.7	5	2.3	4
7	5.4	3	8.0	2	8.6	1	4.7	4	2.5	5
8	2.4	4	2.8	2.5	2.8	2.5	2.0	5	3.4	1
9	6.1	1	3.2	5	3.6	4	4.2	2	3.9	3
10	7.8	1	4.4	2	2.4	4	2.8	3	2.2	5

Beginning with Table VII, we find six out of ten subjects suffering most under the Four-Place Numbers condition and another subject who is equally disturbed by Four-Place Numbers and Digits. Two suffer most after Mental Multiplication and one after Pictures. In other words, there is plain evidence of a greater amount of inhibition after Four-Place Numbers, but no marked differences are apparent among the other conditions. Four subjects are least affected by Reading, two each by Mental Multiplication and Pictures, and one by digits. Although Reading shows least inhibition, from a consideration of this table alone, there is little basis for differentiating among the conditions other than Four-Place Numbers.

According to Table VIII, six individuals out of ten are most affected by Four-Place Numbers, two by Pictures, and one each by Digits and Reading. Two each are least affected by Four-Place Numbers, Digits, Pictures, and Reading. One subject recalled with equal accuracy after Mental Multiplication and Reading, and another with equal accuracy after Reading and Pictures. Here again Four-Place Numbers cause appreciably more retroactive inhibition than the other conditions and again there are few or no apparent differences among those other conditions.

Table IX shows five subjects most inhibited by Four-Place

Numbers, two each by Mental Multiplication and Reading and one by Digits. Three subjects are least inhibited by Reading, two each by Mental Multiplication and Pictures, and one each by the other two conditions. One subject is equally little affected by Digits and Multiplication.

Digits show some tendency to rank second in degree of inhibition caused. This is especially marked in Table VII.

SUMMARY

Experiment II, like Experiment I, shows that there is decided retroactive inhibition where a second series of four-place numbers is memorized between the memorizing and recall of a similar original series. Among the other conditions, Reading shows the least inhibition. Although the odds in its favor are not great, the fact that it is the most advantageous condition in both experiments indicates that its superiority is probably real and not a matter of chance.

Memorizing columns of thirty-two digits causes more inhibition than observing pictures or engaging in mental multiplication. But it causes decidedly less than memorizing a second series of four-place numbers. The first experiment, in its comparison of the retroactive effects of memorizing numbers with those of multiplication, brought out the fact that the degree of retroactive inhibition is a function of similarity of process as well as similarity of content—that similar content in original memorizing and interpolation does not in itself guarantee any more inhibition than is obtained where these contents are comparatively dissimilar. The present experiment also shows but a small degree of inhibition for multiplication, even though, in this instance, the multiplication is to some extent a memorial process. The results of interpolating the memorization of vertical columns of digits add the further important fact that the degree of retroactive inhibition is markedly influenced by similarity in the forms of interpolated and originally memorized materials, as well as by similarity in their contents or in the processes involved. This finding is the more surprising when one recalls that nothing was said to prevent the subjects from learning the digits in groups of fours, if they wished to do so.

EXPERIMENT III

In the experiments so far reported there was plain evidence of retroactive inhibition where original memorizing and interpolation were very similar, and slight evidence of such inhibition for certain conditions where original memorizing and interpolation were comparatively dissimilar. The present experiment was planned to subject these findings to further test.

The writer planned and supervised this investigation. The actual experimentation was carried on by Mr. L. W. Muntz, a graduate student in psychology in Yale University.

The general procedure was suggested by DeCamp's Experiment XIII.[15] He, as the reader will remember, was concerned only with the retroaction caused by dissimilar interpolation. In the experiment to which I refer he studied the effect of addition and other simple arithmetical operations upon the retention of a particular setting or form of chess men. Five men were used: a pawn, a knight, a bishop, a rook, and a queen. These men were placed in a chance arrangement upon a chess board and the subject was allowed fifteen seconds to study them. There followed a three-minute interval given over to (1) Rest (doing nothing) or to (2) Work (solving arithmetical problems for two minutes and resting for one minute). At the expiration of this interval the subject tried to reconstruct the form which he had previously studied. Time for this reconstruction was taken with a stop-watch. To quote DeCamp, "The error value for any chessman, in the reconstructed form, was obtained, speaking mathematically, by adding the differences between the ordinates and abscissae in the two cases, chess-board squares considered as units." DeCamp failed to find any evidence for retroaction in this experiment.

MATERIALS, METHODS AND SUBJECTS

Six chess-men were used in our experiment: king, queen, knight, rook, castle, and pawn. Half of the subjects worked with a chess-board 14 x 14 inches and half with a board 18 x 18 inches, but in each case the men used were proportionate in size to the board. An ordinary focusing cloth was so arranged that, by pulling a string, it could be made to hide the board or uncover it. A stop-watch was used for all timing.

---

[15] Op. Cit. pp. 57-60.

The men were given a chance arrangement on the board and then the subject was given one minute during which to study them. As soon as the minute was up the experimenter directed the subject (these directions took from five to seven seconds) to engage in one of the following interpolated activities for one minute:

(A) *Chess*—Studying another setting of chess-men, or as in the majority of cases, reconstructing a setting learned in a previous experiment of the same day.

(B) *Multiplication*—Performing mentally two-place by two-place multiplications.

(C) *Reading*—Reading selected passages from *American Government and Politics* by Robinson and Beard.

Immediately after the interpolated period the subject was again presented with the chess board and directed to place the men on the board in their proper places as quickly as possible.

Six subjects, four men and two women, served in the regular experiment. All were university students. The men were graduates and the women undergraduates. They were informed that tests were being made on their memories. Aside from this, they had no knowledge of the purpose of the work.

The subjects served every day for nine successive days. On each day each subject went through all three conditions of the experiment. Between tests for different conditions there were rest periods of about ten minutes, usually devoted to random conversation. Practically all of the experimentation was carried on between six and ten P. M.

The subjects were divided into two groups of three each. The arrangement of the experimental conditions for each subject on each day are shown in Table X. Since the arrangement was the same for each group of subjects, only one group is included in the table.

This arrangement was designed to eliminate inequalities in the

TABLE X

ARRANGEMENT OF EXPERIMENTAL CONDITIONS

EXPERIMENT III

Subject	Order	Day 1	Day 2	Day 3	Day 4	Day 5	Day 6	Day 7	Day 8	Day 9
1	1	Cond. A	Cond. B	Cond. C	Cond. B	Cond. C	Cond. A	Cond. C	Cond. A	Cond. B
	2	" B	" C	" A	" C	" A	" B	" A	" B	" C
	3	" C	" A	" B	" A	" B	" C	" B	" C	" A
2	1	" B	" C	" A	" C	" A	" B	" A	" B	" C
	2	" C	" A	" B	" A	" B	" C	" B	" C	" A
	3	" A	" B	" C	" B	" C	" A	" C	" A	" B
3	1	" C	" A	" B	" A	" B	" C	" B	" C	" A
	2	" A	" B	" C	" B	" C	" A	" C	" A	" B
	3	" B	" C	" A	" C	" A	" B	" A	" B	" C

practice to which the various conditions or sequences of conditions were subjected. It may be said here, however, that our results show that there was, in reality, little need for this precaution. The records for the individual subjects seem practically independent of the positions which they occupied in their groups. Besides the arrangement of subjects and conditions as shown in Table X, there was also a methodical arrangement of the particular chance settings given the chess-men, so that each interpolation acted an equal number of times upon each setting.

Results

Table XI gives the accuracy of each subject's reconstruction after the different kinds of interpolation. Each reconstruction is entered simply as C, if correct, and as E, if erroneous. Four subjects out of six give most correct reconstructions after Reading, one shows greatest accuracy after Multiplication, and one shows equal accuracy after Reading and Multiplication. Five out of six subjects give fewest correct reconstructions after Chess and one is equally inaccurate after Chess and Multiplication. Three out of six subjects show an accuracy after Multiplication lower than after Reading and higher than after Chess. Considering the data as a whole, *forty-nine out of fifty-four reconstructions are correct after Reading, forty-one out of fifty-four after Multiplication, and twenty-three out of fifty-four after Chess.* Retroactive inhibition is plainly at work under both the Chess and the Multiplication conditions, but a greater effect is evident in the case of Chess.

Table XII gives the same results as Table XI, but in even clearer terms. Under the day columns are entered the rankings of the three conditions according to the amount of retroactive inhibition shown by the time criterion. Where a given condition is entered as 3, it showed the least inhibition of the three conditions on that day; where it is entered as 1, it showed the most. Again one can make the generalization that Chess causes the greatest, Multiplication the next greatest, and Reading the least amount of retroactive inhibition. Every subject, without exception, gives

TABLE XI

CORRECT AND ERRONEOUS RECONSTRUCTIONS
C = Correct E = Erroneous

Condition	Day 1	Day 2	Day 3	Day 4	Day 5	Day 6	Day 7	Day 8	Day 9	Total Correct Reconstructions
Subject 1										
Reading	E	C	C	C	C	C	C	C	C	8
Multiplication	C	E	E	C	C	C	C	E	C	6
Chess	E	E	E	E	E	E	E	C	C	2
Subject 2										
Reading	C	C	C	C	C	C	C	C	C	9
Multiplication	C	E	C	C	E	C	C	C	C	7
Chess	C	C	C	E	C	C	C	E	C	7
Subject 3										
Reading	C	C	C	C	C	C	C	C	C	9
Multiplication	C	C	C	C	C	C	C	C	C	9
Chess	E	E	C	E	C	E	C	E	C	4
Subject 4										
Reading	C	E	C	C	C	C	E	C	C	7
Multiplication	E	C	C	C	C	C	C	E	E	6
Chess	E	C	E	C	E	C	E	C	E	4
Subject 5										
Reading	C	C	C	C	E	C	C	E	C	7
Multiplication	C	C	C	C	C	C	C	C	C	9
Chess	C	C	C	E	C	E	E	C	E	5
Subject 6										
Reading	C	C	C	C	C	C	C	C	C	9
Multiplication	C	E	E	E	E	C	C	E	C	4
Chess	E	E	E	E	E	C	E	E	E	1

the conditions this order. In terms of composite results, it may be said that:

Chess shows most inhibition in 49 out of 54 comparisons.
Multiplication shows next most in 41 out of 54 comparisons.
Reading shows least in 42 out of 54 comparisons.

RESULTS WITH A CHESS PLAYER

The subjects of our regular experiment were not chess players. We obtained three measurements under each condition, however, for a subject who was an enthusiastic and fairly skilled follower of the game. Of the nine reconstructions made by this subject, all were correct—none of the interpolations disturbed the accuracy of his recall. He also completed his reconstructions very

TABLE XII

Relative Amounts of Inhibition According to the Time Criterion

Condition	DAY 1	DAY 2	DAY 3	DAY 4	DAY 5	DAY 6	DAY 7	DAY 8	DAY 9	Summary
				Subject 1						
Reading	2	3	3	2	3	3	3	3	3	3d 7 times
Multiplication	3	2	2	3	2	1	2	2	2	2d 6 "
Chess	1	1	1	1	1	2	1	1	1	1st 8 "
				Subject 2						
Reading	3	3	3	3	3	3	2	3	3	3d 8 "
Multiplication	2	2	2	2	1	2	3	2	2	2d 7 "
Chess	1	1	1	1	2	1	1	1	1	1st 8 "
				Subject 3						
Reading	3	3	3	3	2	3	2	3	3	3d 7 "
Multiplication	1	2	2	2	3	2	3	2	2	2d 6 "
Chess	2	1	1	1	1	1	1	1	1	1st 8 "
				Subject 4						
Reading	3	1	3	3	2	3	3	3	3	3d 7 "
Multiplication	2	2	2	2	3	2	2	2	2	2d 8 "
Chess	1	3	1	1	1	1	1	1	1	1st 8 "
				Subject 5						
Reading	3	3	3	3	1	3	3	2	3	3d 7 "
Multiplication	2	2	2	2	2	2	2	3	2	2d 8 "
Chess	1	1	1	1	3	1	1	1	1	1st 8 "
				Subject 6						
Reading	3	3	3	2	3	2	2	3	3	3d 6 "
Multiplication	2	2	2	3	2	3	3	2	2	2d 6 "
Chess	1	1	1	1	1	1	1	1	1	1st 9 "

quickly. His times range from 6 to 11 seconds, while those of the inexperienced group range from 5 to 312 seconds. His average reconstruction time is considerably below that of the best member of the group. There is some evidence that his reconstruction time was prolonged most by the interpolation of Chess and next to most by Multiplication. Table XIII shows the rankings of the three conditions according to the inhibition which they showed by the time criterion, with this subject.

TABLE XIII

CONDITION	DAY 1	DAY 2	DAY 3
Chess	1.5	1.0	2.0
Multiplication	1.5	2.5	1.0
Reading	3.0	2.5	3.0

On the whole, the signs of inhibition are not as clear in the case of this subject, accustomed to chess situations, as they are in the case of the inexperienced group.

This fact may be due to the subject's practice with the material involved, or the same native peculiarity which made him take up chess may here have operated to prevent inhibition. From a slightly different point of view, the low susceptibility to retroaction may be due to the comparative isolation of memorizing and interpolated activities in this subject, or he may simply have mastered the original material with unusual thoroughness and thus reduced its likelihood of being disintegrated. All of these factors may have entered into the situation.

In as much as there is only one skilled player to compare with the unskilled group, these findings should be accepted with some reservation.

SUMMARY

The results of Experiment III throw interesting light upon those which have gone before it. Experiments I and II showed that interpolation similar to the original memorizing causes pronounced inhibition, but they did not give any certain evidence of significant retroaction where the interpolation is comparatively dissimilar to the original learning. Experiment III has again demonstrated the greater inhibitory effects of very similar interpolation. It has demonstrated further that there may be (as in the Multiplication condition) marked inhibition where interpolation and original memorizing are comparatively dissimilar. Our results with a practiced chess player give some indication that materials with which one is accustomed to deal are less susceptible to retroactive effects than unfamiliar materials.

CHAPTER III

THE TEMPORAL POSITION OF RETROACTIVE INTERPOLATION

EXPERIMENT IV

Those theories of retroactive inhibition which postulate the interruption of a setting-in process, are based upon the alleged fact that the more closely a retroactive interpolation follows the original learning, the greater is the resulting inhibition. Experiment IV was conducted in order to ascertain whether this fact could be verified or, if not, what relationship there might be between the temporal position of retroactive interpolation and the degree of inhibition caused by it.

MATERIALS, METHODS AND SUBJECTS

Lists of ten three-place numbers were used for the original memorizing. These lists were constructed according to the rules followed for the four-place numbers of the first and second experiments. The following numbers were not used: 135, 357, 579, 246, and 468.

The numbers were displayed on a hand-operated drum which the experimenter controlled with reference to the swings of a pendulum hung on the wall behind the subject. Each number was exposed for a period of two seconds. Twenty seconds elapsed between the disappearance of the last number of one complete exposure of the list and the appearance of the first number of the next complete exposure. Each list was given eight complete exposures. Between the disappearance of the last number of the last complete exposure and the test for recall there was an interpolated period twenty minutes and twenty seconds in length. The subject's activity during the first twenty seconds of this period was uncontrolled. The following twenty minutes were spent in one of five ways:

(A) The whole period was devoted to reading, with no attempt to memorize, in current editions of the *Chicago Tribune.*

(B) The first five minutes were devoted to studying a second list of ten three-place numbers (the list was exposed eight times under the same conditions as the original list), and the following fifteen minutes to newspaper reading.

(C) The first five minutes were devoted to newspaper reading, the next five minutes to studying a second list of numbers, and the remaining ten minutes to newspaper reading.

(D) The first ten minutes were devoted to newspaper reading, the next five minutes to studying a second list of numbers, and the remaining five minutes to newspaper reading.

(E) The first fifteen minutes were devoted to newspaper reading, and the remaining five minutes to studying a second list of numbers.

For the sake of brevity these conditions will be referred to in subsequent discussion simply as A, B, C, D, and E.

Immediately after the interpolated period the subject was directed to reproduce the original list of numbers. This reproduction, as in the first two experiments, was written. Following the reproduction of the original list, the subject, under the B, C, D, and E conditions undertook the reproduction of the interpolated list. The directions for reproduction were the same as in Experiments I and II.

Five subjects served in Experiment IV. All were men. Four were graduate students in psychology at the University of Chicago. One was an undergraduate in the same institution. None of the subjects of this experiment had served in my earlier experiments and none knew the purpose of this work, aside from what might have been inferred from the general procedure.

Five practice sittings for each subject preceded the experiment proper. At these sittings the subject studied a list of ten three-place numbers under the same exposure conditions as those used in the regular experiment. Recall came immediately after the last exposure of the list.

Each subject had thirty-five sittings during the experiment proper—seven under each of the five conditions. A subject went through but one condition on any single experiment day.

The experiment was made up of seven five-day cycles, during each of which all the subjects went through all the conditions once. The experiment was conducted from Monday to Friday inclusive of eight successive weeks, the first week being devoted to the practice sittings.

Each subject went through the five experimental conditions in different order. This arrangement was exactly analogous to that employed in Experiments I and II. Within each cycle the subjects had regular sitting times which were kept as nearly constant as possible.

The criteria of recall were the same as those employed in Experiments I and II, *i. e.*, (1) Amount, (2) Error, and (3) Time.

RESULTS

Table XIV gives the average amount, error, and time of recall after the different interpolated conditions. The table was made up by averaging the average performances of the five subjects under the different conditions.

TABLE XIV

CONDITION	AMOUNT	ERROR	TIME
A	29.1 digits	6.1%	1.6 Secs.
B	18.6 "	31.9%	5.9 "
C	19.7 "	29.0%	5.5 "
D	19.2 "	32.6%	6.4 "
E	18.4 "	30.3%	6.9 "

Condition A, in which the entire interpolated period was spent in newspaper reading, shows, by all criteria of recall, the greatest efficiency, and this by unambiguous amounts. The values under consideration give no indication that inhibition is greatest where the retroactive interpolation follows most closely after memorizing. By amount and error of recall, efficiency is practically equal for the B condition, where the retroactive interpolation occurs earliest in the interpolated period, and for the E condition, where it occurs latest. By the time criterion, recall is more efficient under the B condition than it is under E. Among the conditions other than A, the highest efficiency is under C, where the second number list was studied toward the middle of the interpolated period. That this superiority of C is anything more than accident, however, seems doubtful in light of the small margin of its superiority and the fact that the other condition, namely D, where the retroactive interpolation lies toward the middle of the interpolated period, shows no clear evidence of being better than B and E.

Tables XV, XVI, and XVII, made up in the same way as Tables III, IV, V, VII, VIII, and IX of Chapter II, present the records of the individual subjects.

TABLE XV

AMOUNT OF RECALL

(Total Digits)

	Condition A		Condition B		Condition C		Condition D		Condition E	
SUBJECT	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS
1	27.1	5	15.7	4	15.0	2	15.4	3	14.6	1
2	29.6	5	18.4	4	17.6	2	17.2	1	18.1	3
3	30.0	5	25.3	1	27.4	2.5	27.4	2.5	29.1	4
4	30.0	5	26.9	2	29.7	4	28.8	3	23.4	1
5	28.7	5	6.9	1.5	9.0	4	7.3	3	6.9	1.5

TABLE XVI

ERROR IN RECALL

(% of Recall Erroneous)

	Condition A		Condition B		Condition C		Condition D		Condition E	
SUBJECT	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS
1	18.4	5	45.9	4	48.1	3	57.3	1	51.1	2
2	2.0	5	48.7	1	37.6	3	41.1	2	34.9	4
3	0	5	23.6	3	16.7	4	28.9	1	24.7	2
4	7.1	5	35.6	1	28.7	2	20.0	3	19.3	4
5	2.9	5	5.6	4	13.7	3	15.7	2	21.6	1

TABLE XVII

TIME OF RECALL

(Av. Time in Secs. per Digit)

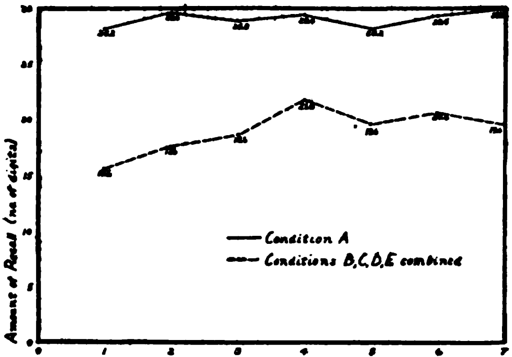
	Condition A		Condition B		Condition C		Condition D		Condition E	
SUBJECT	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS	AVERAGES	RANKS
1	1.5	5	1.9	4	3.0	1	2.6	3	2.8	2
2	1.9	5	4.9	2	5.3	1	4.7	3.5	4.7	3.5
3	1.6	5	8.6	3	6.8	4	10.5	2	11.6	1
4	1.7	5	9.4	2.5	8.9	4	9.4	2.5	9.8	1
5	1.3	5	4.9	2	3.5	4	4.6	3	5.4	1

According to amount of recall (Table XV), Condition A shows the least inhibition for all five subjects. Among the other conditions there is no more differentiation than might be looked for from chance. The same things are true according to error and time of recall (Tables XVI and XVII). From these results the degree of retroactive inhibition seems to be independent of the temporal position of retroactive interpolation.

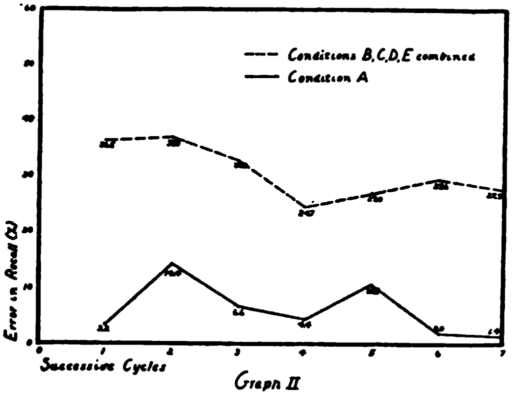
THE INFLUENCE OF PRACTICE UPON RETROACTIVE INHIBITION

The results secured with a practiced chess player in Experiment III indicated that one who is accustomed to dealing with a given material may not be as susceptible to retroactive effects, so far as that material is concerned, as one who has not had such past experience. The present experiment furnishes additional evidence on this point. The average performance of the group under Condition A was calculated for each successive cycle. The average performance under B, C, D, and E combined was calculated in the same way. Graphs I, II, and III show by the three criteria of recall the progress made by the group during the seven cycles of the experiment in memorizing and recalling three-place numbers where (1) retroactive interpolation was used and (2) retroactive interpolation was not used. The amount of inhibition for any cycle is equal to the distance between the curves at that cycle.

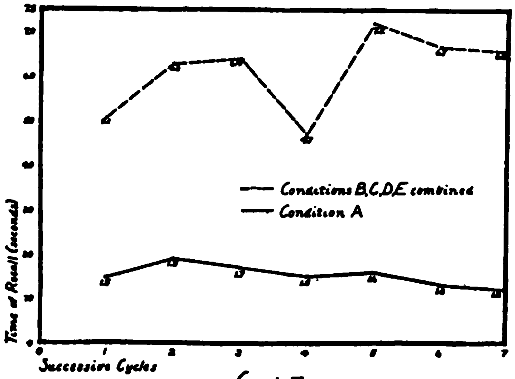
Graphs I and II show a slight tendency for inhibition to decrease as the subjects become more familiar with their task of memorizing. Graph III indicates just the opposite. The lack of clear evidence for decreasing inhibition with increasing practice may be explained in several ways: (1) It may be that no lowering of susceptibility to retroactive inhibition is possible with material of this kind learned under these conditions and subjected to this sort of interpolation. (2) The flatness of the curves suggests that the limit of progress in this function may have been so nearly reached before the beginning of the experiment that no changes in susceptibility to inhibition are present.



Graph I



Graph II



Graph III

(3) Perhaps five cycles are not enough to make apparent changes in susceptibility which are, nevertheless, taking place.

SUMMARY

Under the conditions of this experiment, at least, the degree of retroactive inhibition is independent of the temporal position of retroactive interpolation.

During the seven cycles of the experiment, the five subjects employed give but little evidence of becoming more or less susceptible to retroactive inhibition. The odds are slightly in favor of a lowering of susceptibility with increasing practice.

CHAPTER IV

THE DEGREE OF LEARNING AND SUSCEPTIBILITY TO RETROACTIVE INHIBITION

EXPERIMENT V

The work of Heine, reviewed in the opening chapter of this paper, gave evidence that susceptibility of material to retroactive inhibition stands in an inverse relationship to the amount of study given that material—amount of study being measured in terms of number of exposures. Further study of this question seemed desirable for two reasons: (1) Heine's interpolated work was comparatively dissimilar to her original learning, a condition which my experiments have shown does not give the most striking instance of retroactive inhibition, and (2) In any single experiment she worked with but two degrees of learning, so that no continuous relationship could be shown between varying degrees of learning and varying degrees of retroactive inhibition.

MATERIALS, METHODS AND SUBJECTS

The material for original learning again consisted of lists of ten three-place numbers. These lists were exposed four, six, eight, ten, or twelve times by means of the apparatus used in Experiment IV. The subject was always informed beforehand how many exposures were to be given. Each number was exposed for two seconds. Twenty seconds elapsed between successive exposures of the whole list.

An interpolated period of five minutes and thirty-five seconds occurred between the last exposure of the original list and its recall. This interval was spent in either of two ways which, for convenience, will be designated as Rest and Work. Under the Rest condition the subject's activity was uncontrolled for the first thirty-five seconds; during the remaining five minutes he read, with no attempt to memorize, in current editions of the

Chicago Tribune. Under the Work condition activity during the first thirty-five seconds was again uncontrolled; during the remaining five minutes the subject studied a second list of ten three-place numbers, the list being exposed eight times under the same conditions as the original list.

Immediately after the interpolated period, the experimenter directed the subject to recall the original list of numbers. These directions and the mode of reproduction were the same as in the other experiments with numerical memory material. Under the Work condition the subject recalled the interpolated list following the recall of the original list.

Five subjects, three men and two women, served in the experiment. Two of the men were graduate students in psychology at the University of Chicago. One man was an undergraduate there. The two women were graduate students in psychology at the same institution. The subjects were not informed as to the purpose of the experiment and they were asked not to discuss the work among themselves.

Five practice sittings preceded the regular experiment. At each of these each subject studied a list of ten three-place numbers exposed eight times. This list was recalled after a five-minute period of newspaper reading.

The experiment was made up of two cycles of ten experiment days each. The second cycle was a repetition of the first. The experiment proper was conducted from Monday to Friday, inclusive, of four successive weeks. On each experiment day each subject was tested under both Rest and Work conditions for a single degree of learning or number of repetitions of the original list. Between the tests under Rest and Work conditions there was a period of ten minutes devoted to casual conversation or newspaper reading. Table XVIII gives the arrangement of subjects and experimental conditions for a single ten-day cycle.

One point in this procedure may seem questionable, *i. e.* the fact that all subjects were tested first under the Work condition during the first half of each cycle and first under the Rest condition during the second half of each cycle. Obviously the pro-

TABLE XVIII

Exs. = Exposures

ARRANGEMENT OF EXPERIMENTAL CONDITIONS

EXPERIMENT V

Subject	Order	Day 1	Day 2	Day 3	Day 4	Day 5	Day 6	Day 7	Day 8	Day 9	Day 10
1	1	4 Exs. Work	6 Exs. Work	8 Exs. Work	10 Exs. Work	12 Exs. Work	4 Exs. Rest	6 Exs. Rest	8 Exs. Rest	10 Exs. Rest	12 Exs. Rest
	2	4 Exs. Rest	6 Exs. Rest	8 Exs. Rest	10 Exs. Rest	12 Exs. Rest	4 Exs. Work	6 Exs. Work	8 Exs. Work	10 Exs. Work	12 Exs. Work
2	1	6 Exs. Work	8 Exs. Work	10 Exs. Work	12 Exs. Work	4 Exs. Work	6 Exs. Rest	8 Exs. Rest	10 Exs. Rest	12 Exs. Rest	4 Exs. Rest
	2	6 Exs. Rest	8 Exs. Rest	10 Exs. Rest	12 Exs. Rest	4 Exs. Rest	6 Exs. Work	8 Exs. Work	10 Exs. Work	12 Exs. Work	4 Exs. Work
3	1	8 Exs. Work	10 Exs. Work	12 Exs. Work	4 Exs. Work	6 Exs. Work	8 Exs. Rest	10 Exs. Rest	12 Exs. Rest	4 Exs. Rest	6 Exs. Rest
	2	8 Exs. Rest	10 Exs. Rest	12 Exs. Rest	4 Exs. Rest	6 Exs. Rest	8 Exs. Work	10 Exs. Work	12 Exs. Work	4 Exs. Work	6 Exs. Work
4	1	10 Exs. Work	12 Exs. Work	4 Exs. Work	6 Exs. Work	8 Exs. Work	10 Exs. Rest	12 Exs. Rest	4 Exs. Rest	6 Exs. Rest	8 Exs. Rest
	2	10 Exs. Rest	12 Exs. Rest	4 Exs. Rest	6 Exs. Rest	8 Exs. Rest	10 Exs. Work	12 Exs. Work	4 Exs. Work	6 Exs. Work	8 Exs. Work
5	1	12 Exs. Work	4 Exs. Work	6 Exs. Work	8 Exs. Work	10 Exs. Work	12 Exs. Rest	4 Exs. Rest	6 Exs. Rest	8 Exs. Rest	10 Exs. Rest
	2	12 Exs. Rest	4 Exs. Rest	6 Exs. Rest	8 Exs. Rest	10 Exs. Rest	12 Exs. Work	4 Exs. Work	6 Exs. Work	8 Exs. Work	10 Exs. Work

cedure would have been better balanced if there had been a second group of subjects for whom the order of Rest and Work conditions was reversed. The comparative flatness of the practice curves for learning three-place numbers, however, (see Graphs I, II, and III) suggests that the apparent discrepancy is logical rather than actual.

The criteria of recall were: (1) Amount, (2) Error, and (3) Time.

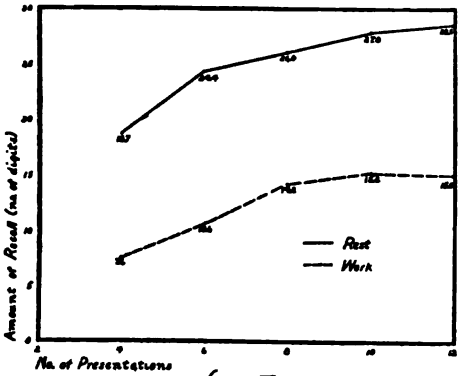
RESULTS

Graphs IV, V, and VI are curves showing amount, accuracy, and time of recall for the different degrees of learning where (1) Rest—newspaper reading—and (2) Work—memorizing numbers—were interpolated. These curves were made up from the averages of the entire group of subjects for the different conditions.

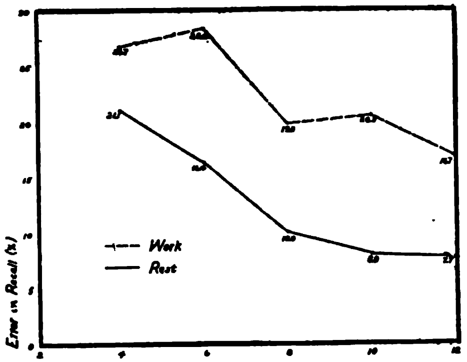
By amount of recall (Graph IV) both Rest and Work curves show a regular, negatively accelerated rise with increases in the number of exposures of the original list. And the distance between the curves, which is a measure of the degree of retroactive inhibition, remains practically constant. If the better learned lists are less susceptible to retroactive effects, this is not evident within the limits studied which, so far as amount of recall is concerned, extend almost to a point of complete learning.

The error curves (Graph V) are more difficult to interpret. Accuracy of recall after Work changes very irregularly with changes in the number of presentations. It is possible that the memorized material's susceptibility to disintegration actually varies in the manner suggested by the curve, but it seems more probable that the variability of efficiency under the Work condition is a matter of the limited sampling which it represents. In the latter event, since the Rest and Work curves show no continuous tendency to converge or diverge, it may be concluded that, as in the case of amount of recall, there is a practically constant susceptibility to inhibition for the different degrees of learning employed in this experiment.

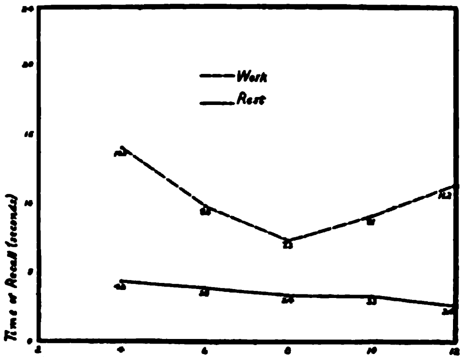
According to the time curves (Graph VI) progress under the



Graph IV



Graph V



Graph VI

Rest Condition is regular and almost linear. Under the Work condition, however, the curve falls abruptly to the eight presentations point and then rises with almost equal abruptness to twelve presentations. Here again the irregularity of the Work curve may be interpreted as due to the real nature of the function represented or to the limited sampling. In either case one can say that, from four up to twelve presentations of the memorized material, there is but little evidence for a single continuous change in susceptibility to inhibition.

Perhaps the safest generalization to be made upon the basis of the three pairs of curves is the rather negative one, that, although there are sporadic increases and decreases in susceptibility to inhibition with varying numbers of presentations, there is no continuous tendency in one direction or the other. From a theoretical standpoint it would be futile to accept such a statement as characterizing retroactive inhibition for all degrees of learning. In doing so we should fly in the face of a respectable body of empirical facts concerning the isolation, through practice, of interfering processes. We should have to deny the validity of Heine's findings, a thing which the general character of her work would hardly justify. My results here do indicate, however, that within fairly wide limits a complex set of associations may show no uniform variation in susceptibility to disintegration.

According to two out of three criteria, progress in efficiency with increasing presentations is much more regular under the Rest than under the Work condition. This in itself is important. Evidently retroactive inhibition, as observed in this experiment, is a highly variable phenomenon.

So far I have been speaking of absolute amounts of inhibition. Table XIX gives the relative amounts of loss due to the interpolation of Work. Each value was obtained by dividing the difference in efficiency between Rest and Work by the efficiency under the Rest condition. Two sets of values are given for errors: (1) relative inhibition according to percentages of error, and (2) relative inhibition according to gross errors. Up to this point percentages of error alone have been considered. Since a more or less valid objection can be raised, however, to the procedure of figuring one set of relatives from another set of relatives, I have included in this table the set of relative error values based upon gross errors, *i. e.,* the actual number of errors made under the different conditions.

TABLE XIX

Relative Inhibition

PRESENTATIONS	AMOUNT	TIME	% ERROR	GROSS ERROR
4	59.3	233	26	—51
6	56.6	151	73	—25
8	45.4	115	98	+ 8
10	45.3	176	156	+41
12	47.3	367	117	+14

The general tendency, according to amount of recall, is toward relatively less inhibition with increasing numbers of presentations. True, there is more inhibition at twelve presentations than at ten or eight, but the difference is hardly great enough to be significant. In the main, there is a rapid decrease in relative inhibition at first and then a period where neither increase nor decrease is clearly marked.

The time criterion indicates no single continuous variation in the degree of relative inhibition. The values decrease rapidly from the four to eight presentations point and then increase rapidly up to the twelve presentations point.

Both sets of error values show, in general, an increase in relative inhibition with increasing presentations. In both cases there is less inhibition at twelve than at ten presentations, but this might fairly be interpreted as an accidental irregularity.

It is interesting to note the two minus values under gross errors. They indicate that the actual number of errors made at four and six presentations was less under the Work than under the Rest condition. In this sense, there is retroactive facilitation rather than inhibition at these points.

On the whole there is little agreement among the criteria as to any continuous correlation between the degrees of learning examined and relative inhibition. This may be due in part to the fact that the methods of determining relative inhibition must by the nature of the case, be quite arbitrary. From the same data as those employed in calculating relative inhibition according to error of recall, one could calculate relative inhibition according to accuracy of recall. Such a procedure would change appreciably the relative values for different numbers of presentations and also the relationship between these relative values. In fact, it would tend, at least, to invert the relationship. The same can be said for the amount criterion. If the perfectly legitimate procedure were adopted of scoring amount of recall in terms of the number of digits recalled less than the total possible number, then the present relationship between the number of presentations and relative inhibition according to amount of recall would tend to be inverted.

SUMMARY

In light of the fact that relative degrees of inhibition are functions of arbitrary scoring methods, it seems to me that my most significant findings are: (1) The lack of any single continuous change in absolute susceptibility to inhibition within the limits studied, and (2) The comparative irregularity, under the Work condition, of the progress in efficiency of recall derived from given increments of presentations. It is evident that much further experimentation must be carried out in connection with this problem before any very definite generalizations can be made.

CHAPTER V

SUMMARY AND DISCUSSION OF RESULTS

The results of the experiments reported in this paper stand in rather sharp disagreement with much of what has previously been said on the subject of retroactive inhibition. This may be due to the fact that my methods and materials were, for the most part, quite unlike those of other investigators in this field. Such differences in method, however, should increase rather than decrease the value of these new results for the formulation of a conception of retroactive inhibition which is to have applicability beyond the activity of memorizing some particular material, such as nonsense syllables.

My results may be briefly summarized as follows:

(1) The degree of retroactive inhibition present in a given situation is a function of the similarity between interpolated activity and original learning. This relationship cannot, of course, be thought of as linear. That is, if an exact measure of similarity were available and the degrees of retroactive inhibition were plotted against varying values of this similarity, we should not find a steady increase in inhibition values with increases in similarity. Such an assumption would require the expectation of greatest inhibition where similarity is greatest, *i. e.,* where the interpolation and original learning are the same. But we know that in such cases there is facilitation due to practice, which excludes by definition any detectable inhibition. Under the conditions of these experiments, except in the cases of isolated individuals, the greatest inhibition was always caused by the interpolations most similar to the original learning, but further work will have to decide at just what point in the variation of similarity interpolation turns from a disintegrating into an integrating agency.

There is also the possibility that similarity is not a one-dimensional affair. Indeed my results bring out the fact that there may be effective similarity of contents, of forms of presentation, or of processes involved, and variations in any one of these may independently modify the degree of retroactive inhibition. A further complicating factor may be present in the quantitative character of these variations. Certain of them may be serial and continuous, each change in that type of similarity being accompanied by a change in the degree of inhibition. Others may be of the *all or none* variety. For example, the processes involved may be identical or disparate. A certain amount of inhibition may be present with such identity, while, without it, there may be no inhibition caused by process similarity. That is to say, there need be no intermediate stages of similarity, so far as retroactive inhibition is concerned, between identical and plainly dissimilar processes. If the interpolated activity is memorial there may be, by virtue of this fact alone, appreciable inhibition. All other factors being ineffective, the fact that the interpolated activity is not memorial, in the ordinary sense of that term, may exclude the possibility of inhibition.

Of course this discussion is of a highly hypothetical sort. Exact knowledge of the relationship between retroactive inhibition and similarity or similarities between interpolation and original learning will have to wait upon future researches. The positive contribution of my results in connection with this point lies in their clear indication of the simple fact that similarities of contents, of processes, and of forms of presentation all play a part in determining the degree of retroactive inhibition.

(2) Where, on the face of things, interpolation and original learning are comparatively dissimilar there may still be an appreciable degree of retroactive inhibition. Engaging in certain fairly strenuous activities between memorizing and recall results in a lower efficiency of recall than where the subject occupies himself with a comparatively light task, such as newspaper reading with no attempt to memorize. This fact might possibly be interpreted as another instance of the relationship between the similarity of activities and retroactive inhibition. It might be argued, for example, that two activities are similar to the extent

to which they approach intensity equality. It might also be argued that some other unapparent differences in similarity exist between those conditions where there is more or less retroactive inhibition.

(3) While the results of the first three experiments show the greatest inhibition where there is unquestionably the greatest similarity between interpolation and original learning, they also point out the extreme difficulty of predicting the degree of inhibition which an apparent degree of similarity will bring about. The contents of both memorizing and interpolation may be numbers (four-place numbers, indeed), but unless the two are memorized there may be no more inhibition than where one is numbers and the other, say, poetry. Further, the contents of both may be numerical and the process of both memorial, but unless the two sets of material are presented in highly similar form there may be no high degree of inhibition.

(4) The degree of retroactive inhibition is independent of the temporal position of the retroactive interpolation. For the materials used this generalization holds in respect to an interval of twenty minutes between memorizing and recall.

(5) Practice in memorizing a particular kind of material may alter one's susceptibility to retroactive inhibition. A single experienced chess player was less susceptible to retroactive inhibition than a group of less experienced individuals. This may easily have been an accident, or the same factors which made this man a good chess player may have been the cause of his resistance to retroactive inhibition. A group of five subjects who memorized four-place numbers five days a week for eight successive weeks showed slight indications of becoming less susceptible to retroactive inhibition so far as their memory for that kind of material was concerned.

(6) Within the limits studied there is no single, continuous relationship between the number of presentations of a memorized material and its absolute susceptibility to retroactive inhibition. It seems improbable to suppose that this will hold for all possible degrees of learning, and it may not hold where material is

learned in simpler units. In fact, the work of Heine suggests that, where simple units of a complex material are learned in pairs, the degree of inhibition is an inverse function of the degree of learning. My results with the experienced chess player, if they are due to his more perfect mastery of the memorized material within the allotted time, also indicate an inverse relationship between retroactive inhibition and the degree of learning.

There is little or no agreement among the criteria employed as to the relationship between the degree of learning and the relative amount of inhibition.

(7) The increases in efficiency of recall incident upon adding constant increments of exposures is less predictable, according to two out of three criteria, where memorizing is followed by some constant disintegrating activity of great effectiveness than where it is followed by an activity of low disintegrating effects. If the efficiency of recall is plotted against the number of exposures given a material, a comparatively irregular curve will result where the material is subjected to retroactive inhibition, and a comparatively smooth curve for material not so subjected.

THEORETICAL CONSIDERATIONS

The above results, where they apply at all, seem to me to favor a transfer theory of retroactive inhibition. Such a theory, as we saw in the second chapter, usually implies that some elements of the original learning and interpolation are common. The activity of these elements during interpolation may weaken their position in the original pattern, or their activity during interpolation may so modify their associative potentialities that, in recall, they will have a tendency to bring up items irrelevant to the original learning or items which are conflicting and mutually inhibitory. All of these things may happen. Support is given this theory by the fact that, in my experiments, the greatest inhibition always was caused by the interpolation which apparently had the most elements in common with the original learning.

Such a conception gives retroactive inhibition an intelligible setting among the other phenomena of learning. Two facts, however, may seem to stand in the way of its acceptance. In the first place, Müller and Pilzecker found as great inhibition where interpolation was dissimilar to original learning as where it was similar. In the second place, the experiments of Heine and Tolman, as well as those reported here, give ample evidence that retroactive inhibition may occur where two activities are comparatively dissimilar. As far as the results of Müller and Pilzecker are concerned, they may be dismissed as quantitatively inconclusive on this point. While Heine and Tolman found retroactive inhibition where interpolation and original learning were dissimilar, they present no comparisons between the retroactive effects under conditions of greater and less similarity. The facts brought out by the present investigation, in the course of which just such comparisons were made, indicate that retroactive inhibition is decidedly a function of the similarity of two activities and, therefore, probably a matter of transfer.

Still, one can not dismiss the fact that inhibition of plain degree can be caused by interpolated activities having apparently few elements in common with original learning. Here one faces two alternatives: (1) There may be two ways (or more, perhaps) in which interpolated activity positively accelerates the process of forgetting. In certain instances retroactive inhibition may be a kind of transfer. In certain other instances it may be something entirely different. (2) All retroactive inhibition may be a matter of transfer.

Taking the alternative of explaining only certain instances of retroactive inhibition in terms of transfer it is fair to ask what acceptable explanation can be given in other terms. A perseveration theory such as that of Müller and Pilzecker would undoubtedly be advanced from some quarters. Such a theory would explain retroactive inhibition as the effect of interrupting some consolidating or setting-in process going on after conscious memorizing. One test of such a theory is a comparison of the results of interpolation placed in different temporal positions between memorizing and recall. Müller and Pilzecker have some rather meagre evidence (one experiment with one subject) that, if such a supposed process is allowed time to mature, the memorized material will be less easily disintegrated. My Experiment IV showed no such relationship between retroactive inhibition and the temporal position of the retroactive interpolation. It might be said in answer to this that the inhibition I obtained was of the transfer variety, while that of Müller and Pilzecker was not. But transfer or no transfer, if a setting-in process is open to interruption by a dissimilar activity, is there any reason to believe that it is not just as likely to be disturbed by a similar activity?

Another argument against a perseveration theory of this kind is the difficulty of conceiving of it in neurological terms. Of course, it is possible that one group of active nervous elements tends to drain off energy from other simultaneously active elements and so reduces the activity of the latter. But that such a drainage, if it exists at all, is independent of the degree of identity of the groups of elements involved seems quite unlikely.

There remains a consideration of the theory which would explain all retroactive inhibition in terms of transfer. This theory easily explains retroaction where original memorizing and interpolation are plainly similar, but, where they are not, a slight difficulty arises. One must admit that it would be stretching a point to accept the transfer theory, if all similarity were, like the superficially most evident sort, a matter of contents, and if all transfer were a matter of elements active in original memorizing being active, and therefore getting new associative potentialities, during interpolation. There is no need, however, for so limiting one's conceptions of similarity and transfer. The present experiments bring out the effectiveness of other kinds of similarity than that of content, and, as Webb has suggested, transfer is not as restricted a phenomenon as is sometimes supposed. The transfer causing retroaction, for instance, may in one case be a transfer from memorizing to interpolation and in another a transfer from interpolation to recall. All that one need assume, in order to explain any retroaction in terms of transfer, is that the situations, memorizing, interpolation, and recall, have enough in common, through content, form, process, or even temporal contiguity, to insure the reinstatement of a part of one of the situations in intimate connection with another.

Thus it appears that, while many desirable facts are still outstanding, all positive acceleration of the forgetting process due to interpolated mental activity can be explained in terms of transfer. The writer does not feel in a position to say how profitably this theory can be used in connection with retrograde amnesia—the pathological instance of positively accelerated forgetting.

Vol. XXVIII
No. 7

PSYCHOLOGICAL REVIEW PUBLICATIONS

Whole No. 129
1920

Psychological Monographs

EDITED BY
JAMES ROWLAND ANGELL, 522 Fifth Ave., New York
HOWARD C. WARREN, Princeton University (*Review*)
JOHN B. WATSON, New York (*J. of Exp. Psychol.*)
SHEPHERD I. FRANZ, Govt. Hosp. for Insane (*Bulletin*) and
MADISON BENTLEY, University of Illinois (*Index*)

The Higher Mental Processes in Learning

BY
JOHN C. PETERSON, Ph.D.
Assistant Professor of Psychology, State Agricultural College,
Manhattan, Kansas

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

Agents: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W. C.)
Paris (16 Rue de Condé)

Psychological Review Publications

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, 244 MADISON AVE., NEW YORK (*J. of Exp. Psych.*)
JAMES R. ANGELL, 522 FIFTH AVENUE, NEW YORK (*Monographs*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*)
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

PSYCHOLOGICAL BULLETIN

containing abstracts, critical reviews, notices of books and articles, psychological news and notes, university notices, and announcements, appears monthly, the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bimonthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 480 pages.

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in May, and may be subscribed for in connection with the periodicals above, or purchased separately.

THE PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. THE MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.

Philosophical Monographs: a series of treatises more philosophical in character.
Library of Genetic Science and Philosophy: a series of bound volumes.

ANNUAL SUBSCRIPTION RATES

Review: $4.25 (Foreign, $4.50). Review and Bulletin: $9.00 (Foreign, $9.50).
Journal: $4.25 (Foreign, $4.50). Review and Journal: $8.25 (Foreign, $8.75).
Bulletin: $5.00 (Foreign, $5.25). Journal and Bulletin: $9.00 (Foreign, $9.50).
Any one of above with Index: $1.25 additional.
Any two of above with Index: $1.00 additional.

Review, Bulletin, and Journal: $12.50 (Foreign, $13.25).
Review Bulletin, Journal, and Index: $13.50 (Foreign, $14.25).
Current Numbers: Review or Journal, 80c.; Bulletin, 50c.; Index, $1.50.
Psychological Monographs: $5.50 per volume (Foreign, $5.80).
Current Issues: prices vary according to size.

Subscriptions, orders, and business communications may be sent direct to the

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, NEW JERSEY
AND LANCASTER, PA.

FOREIGN AGENTS: G. E. Stechert & Co., London (2 Star Yard, Cary St., W. C.);
Paris (16, rue de Condé)

I. INTRODUCTION

The present investigation is an experimental study of the mental processes involved in the solution of certain novel problems and in the utilization of experiences so gained for the subsequent mastery of other problems of a similar nature. The problems were chosen with a view to the possibility of accurate measurement of the progress in their mastery. They are arranged roughly in the order of increasing complexity, and are so related that the solutions for later problems are, for the most part, simply more generalized statements of solutions for earlier ones. In the mastery of these problems all forms of mental process, from very simple acts of perception to fairly difficult and complicated acts of abstraction and generalization, were involved. It is these latter processes chiefly to which attention is directed in the following pages.

In its use of objective methods the study is closely related to the large experimental literature in the field of animal learning and the so-called lower forms of human learning. Its dominant interest in the processes of abstraction and generalization brings it into relation with the researches of Külpe, Mittenzwei, Grünbaum, Moore, Aveling, and Fisher in this field.[1] Its relation to the more dynamic studies of Cleveland[2] and Ruger is more direct.[3] From them it differs mainly in attempting a more accurate measurement of progress and a more careful discrimination of the elements involved. It differs from Ruger's work also in the fact that no skilled motor coördinations are involved, although overt manipulation and perceptual judgments do continually enter in as factors in the solution of problems.

---

[1] Külpe, Oswald, "Versuche uber Abstraktion," Ber. uber den I Kong. f. exp. Psych. in Geissen, 1904.

Mittenzwei, Kuno, "Uber abstrahierende Apperception," Psych. Stud. 1907, 2.

Grunbaum, A. A., "Uber die Abstraktion der Gleichheit," Arch. f. d. ges. Psychol. 1908, 12.

Moore, Thomas Verner, "The Process of Abstraction: An Experimental Study," University of California Pubs. in Psych., Vol. I.

Aveling, Francis, "The Consciousness of the Universal."

Fisher, Sara Carolyn, "The Process of Generalizing Abstraction," Psych. Rev., Mon. Sup., Vol. XXI, No. 2, 1916. Fisher summarizes the work in generalization and abstraction prior to 1916.

[2] Cleveland, A. A., "The Psychology of Chess and Learning to Play It," Am. Jour. Psych., Vol. 18, 1907.

The portion of Ruger's investigation to which this study is most closely related, is described by its author as follows: "In certain cases the degree of complexity (of puzzles presented to his subjects for solution) could be indefinitely increased, and yet a single rule be developed for the solution of the various resulting forms." It does not appear from his descriptions, however, that the related forms were such as to require progressively higher levels of generalization from problem to problem.

The study of the utilization of experiences gained in earlier problems for the solution of later ones brings our discussion into direct relation with much of the extensive literature on the transfer of training. The experimental work in this field, like that on the process of abstraction and generalization, has been summarized so recently as to obviate the necessity for further summarization at this point.[4]

---

[3] Ruger, Henry Alford, "The Psychology of Efficiency," Columbia Contributions to Philosophy and Psychology, Vol. XIX, No. 2, 1910.

[4] Coover, John Edgar, "Formal Discipline from the Standpoint of Experimental Psychology," Psych. Rev. Mon. Sup., Vol. XX, No. 3, 1916.

II. TECHNIQUE AND METHOD OF PROCEDURE

A. Description and Analysis of the Problems Presented to the Subjects for Solution

The problems whose solution constituted the task of our subjects were found in a simple game which proved susceptible of easy expansion and adaptation to the needs of the present study. The original game calls for the presentation of a number of matches, say 14, from which two persons draw alternately, each being permitted to take 1 or 2 matches at a draw. He who manages to get the last draw wins the game. For simplicity of presentation we shall speak of the two players as the subject (S) and the experimenter (E). In our experiments S was always required to draw first. A string of beads mounted upon a steel wire was substituted for the less convenient matches.

Now when the initial number of beads is 14, S can always win the game by taking 2 beads at his first draw and enough at every draw thereafter to make the sum of each of his own draws and E's preceding draw equal to 3. In this manner he reduces the sum of beads successively to 12, 9, 6, 3, and 0, thereby securing for himself the *last draw* and so winning the game.

The mastery of this simple game constitutes a problem the solution of which involves some learning, as is indicated by the fact that a group of senior college students required 50.6 trials on the average for the winning of two successive trials. Each trial consists of the subject's responses to a series of situations which vary slightly though regularly as will be explained later.[1] The learning process consists in finding the correct response for each situation and in connecting every situation with its appropriate response. When the objective situations and responses are recorded, it becomes possible to trace the progress of learning from trial to trial until any required degree of mastery of the problem is attained.

---

[1] A trial consists of all draws involved in the reduction of any initial number of beads to 0, regardless of whether S wins or loses.

When a single problem of this sort is solved, the solution is usually couched in more or less specific terms which do not readily function in the subsequent solution of other similar problems. In the solving of a series of related problems, however, it is possible to observe the gradual abstraction of common elements and the association of these elements with appropriate terms, leading finally to the formulation of a general principle for the solution of all problems of the series. If the mastery of 14 as the initial number of beads constitutes the first problem, mastery of 15 as the initial number will constitute the second problem, mastery of 16 as the initial number the third, and so on until enough problems have been solved to permit the subject to develop a general formula for his guidance in drawing from any number of beads.[2]

In the first problem the subject learned that he could not win if required to draw from 3, 6, 9, or 12 beads. He also learned that when the initial number of beads was 14, he *could* win by drawing so as to compel his opponent to draw from these *"critical"* numbers. In the later problems of the series he discovers that these numbers are critical (i.e., the one who must draw from them inevitably loses the game) in all problems of the series, regardless of the initial number of beads presented for solution. Here also he generalizes to the effect that all multiples of 3 are critical numbers, and that he can win any number which is not a multiple of 3 by reducing it to such a multiple at his first draw and through successively lower multiples of 3 to 0 at subsequent draws. Aside, then, from the development of specific responses for the solution of individual problems, the learning process here consists in the formation of (1) a general concept through which the essential elements of all problems may be represented by a single term and treated as a unit, and (2) a system of draws under the control of this concept, by means of which all *non-critical* numbers of the series may invariably be won by the subject. The basic concept here developed may be termed the *critical-number* concept. Its functioning and further development through use may be observed by requiring the subject to solve some additional series of problems somewhat similar to those described above.[3]

---

[2] For the sake of uniformity it is necessary to fix upon a definite degree of mastery of problems to be required in all cases. Some difficulty arises here owing to the fact that it is impossible for the subject to win when the initial number of beads presented for solution is a multiple of 3. We have considered such problems "solved" when the subject expressed a conviction that he could not win and called for a new problem. All other problems have been considered solved when two consecutive trials were won.

New series of problems of this sort may be had by varying the numbers of beads which may be taken at a single draw. For example, the extension of the numbers which may be drawn to 1, 2, or 3 (instead of only 1 or 2 as above) yields a new series of problems in which the critical numbers are multiples of 4. Further extension of the numbers which may be drawn to 1, 2, 3, or 4 beads yields another series of problems in which the critical numbers are multiples of 5, etc. By further changes in the numbers which may be drawn an indefinite number of similar series of problems may be obtained. The various series of problems thus obtained will hereafter be designated by the lowest and the highest numbers which may be drawn. Thus *Series 1-2* will denote the series of problems in which only 1 or 2 beads may be taken at a draw; *Series 1-3,* the series in which 1, 2, or 3 beads may be drawn; *Series 2-3,* the series in which only 2 or 3 beads may be drawn, etc.

It will be worth while to direct our attention to the location of critical numbers in the various series since these numbers furnish the key to the solution of the various problems and series of problems. We have observed already that all multiples of 3 are critical numbers in Series 1-2. A clearer insight into the reason why these numbers are critical in this series will facilitate the search for critical numbers in other series of problems. In Series 1-2 any pair of draws may result in the removal of either 2, 3, or 4 beads (the possible combinations of S's and E's draws being 1-1, 1-2, 2-1, 2-2, and the sum of these combinations 2, 3,

---

[3] A series of problems was considered solved when the subject gave an adequate general formula for the solution of all problems of the series or in some other manner clearly indicated his ability to solve all new problems of the series at sight.

3, and 4 respectively). At any draw either S or E can take away such a number as to make the sum of his own draw and the immediately preceding draw of his opponent equal to 3 by taking 1 when the opponent takes 2, and 2 when the opponent takes 1. Thus either S or E can cause the number of beads to be reduced by 3 with each successive pair of draws, beginning with any draw of his opponent, but he cannot force a reduction by 2's or 4's in this manner for if the opponent's draw is 2, the sum of the pair of draws will necessarily be greater than 2, and if the opponent's draw is 1, the sum of the pair will necessarily be less than 4. The only number, therefore, by which successive reductions can thus be forced is 3, and it is clear that 3 is under control in this manner only because it is the sum of the low and the high draw. If, then, we designate the low draw by the letter L and the high draw by H, we may say in more general terms that *all multiples of the sum of $L + H$ are critical numbers.*[4] Notice that these numbers are critical not merely because they are multiples of the sum of $L + H$ but because they are greater than 0 by exact multiples of this sum, and 0 is critical by definition. If the conditions of the game were so altered as to make 1 instead of 0 a critical number, the critical numbers would be represented by the terms of an arithmetical progression in which the common difference is $L + H$ and the first term is 1.[5]

---

[4] That this formula holds for all series of problems of this sort wherein winning consists in securing the last draw, can be shown by substituting the general terms, L and H, for 1 and 2 in the foregoing statement. Thus either S or E can take away such a number of beads at any draw as to make the sum of his own draw and the opponent's immediately preceding draw equal to $L + H$. If one's opponent draws L, one can draw H; if the opponent draws H, one can draw L, and if the opponent draws $L + x$, one can bring the sum of the pair of draws up to $L + H$ by taking away $H - x$ beads. Moreover, $L + H$ is the only number by which successive reductions can thus regularly be forced for if the opponent's draw is L, one cannot draw enough to raise the sum of the pair of draws above $L + H$, and if the opponent's draw is H, one cannot draw so low a number as to make the sum of the pair of draws less than $L + H$.

[5] This condition is actually realized if in Series 1-2, for example, the definition of success is so modified that drawing last constitutes losing instead of winning the game.

In all series of problems in which L is 2 both 0 and 1 may be said to be critical by definition. The critical numbers are here represented by the terms of two arithmetical progressions having each a common difference of L + H. The first terms of these progressions are 0 and 1 respectively. When L is 3, 2 also becomes a critical number. In fact, it will be noticed that all numbers below L, including 0, are in every series critical. These numbers may be called the *basic* critical numbers. Each basic critical number is the first term of an arithmetical progression in which the common difference is L + H and of which all terms are critical numbers. The critical numbers of any series are therefore 0, 1, 2.......L — 1 and all numbers which are greater than any of these basic critical numbers by exactly a multiple of the sum of L + H. This generalization holds for all series in which all numbers between L and H may be drawn. Series of this sort have been called *continuous* series. All series of problems described in the foregoing pages are continuous series. Other series of problems requiring more difficult generalizations may be had by restricting the draws to L and H. Series of this sort in which only L and H may be drawn will be known as *discontinuous* series. Only a limited number of discontinuous series were solved by our subjects, owing to the difficulty of manipulation when the values of L and H are high. To facilitate their analysis these discontinuous series with some of their critical numbers are listed below. Bear in mind that the numbers by which a discontinuous series is indicated are the *only* numbers which may be drawn in that series.

Series (discontinuous).	*Critical Numbers.*
1 or 3;	*0*, 2, *4*, 6, *8*, 10, *12*, 14, *16*, etc.
1 or 5;	*0*, 2, 4, *6*, 8, 10, *12*, 14, 16, etc.
1 or 7;	*0*, 2, 4, 6, *8*, 10, 12, 14, *16*, etc.
2 or 6;	*0,1*, 4,5, *8,9*, 12,13, *16,17*, 20,21, etc.
2 or 10;	*0,1*, 4,5, 8,9, *12,13*, 16,17, 20,21, etc.
3 or 9;	*0,1,2*, 6,7,8, *12,13,14*, 18,19,20, *24,25,26*, etc.
4 or 12;	*0,1,2,3*, 8,9,10,11, *16,17,18,19*, 24,25,26,27, etc.
1 or 4;	*0*, 2, *5*, 7, *10*, 12, *15*, 17, *20*, 22, etc.
1 or 6;	*0*, 2, 4, *7*, 9, 11, *14*, 16, 18, *21*, etc.
1 or 8;	*0*, 2, 4, 6, *9*, 11, 13, 15, *18*, 20, etc.
2 or 5;	*0,1*, 4,–, *7,8*, 11,—, *14,15*, 18,—, etc.

2 or 7; *0,1*, 4, 5, *9,10*, 13,14, *18,19*, 22,23, etc.
2 or 8; *0,1*, 4,5, *10,11*, 14,15, *20,21*, 24, 25, etc.
2 or 9; *0,1*, 4,5, 8,–, *11,12*, 15, 16, 19,—, *22,23*, etc.
3 or 7; *0,1,2*, 6,–,–, *10,11,12*, 16,—,—, *20,21,22*, etc.
3 or 8; *0,1,2*, 6,7,–, *11,12,13*, 17,18,—, *22,23,24*, etc.
4 or 9; *0,1,2,3*, 8,–,–,–, *13,14,15,16*, 21,—,—,—, etc.

The numbers in italics are those obtained by applying our old formula to these series. These numbers will be known as *primary* critical numbers and groups of them as primary C groups. Within the intervals between primary C groups there are now other critical numbers which have become critical because of the restriction of draws to L and H. Such numbers will be known as *secondary* critical numbers. The secondary critical numbers fall into order in accordance with a principle already applied to the primary critical numbers; namely, that there are certain basic critical numbers each of which is the first term of an arithmetical progression in which the common difference is L + H and of which all terms are critical numbers. Our task will therefore be confined to finding the basic secondary critical numbers in the series listed above; i.e., those secondary critical numbers which fall in the interval between the first two groups of primary critical numbers.

Note that in every series in the list *all numbers below H, which may be obtained by adding a multiple of 2L to any number in the primary C group, are critical.* Why are these numbers critical? and why do they not extend beyond H — 1 above 0? Obviously the possibility of using the high draw no longer exists when the number of beads has been reduced below H. Below this point the game must necessarily proceed by successive reductions of L beads at each draw, and each pair of draws will result in the reduction of the number by 2L. Now if, after reduction below H, the number of beads remaining is equal to any number in the primary C group plus a multiple of 2L, it will itself be critical, because the one who draws first in the first pair of draws will necessarily draw first in all succeeding pairs of draws, wherefore the last and winning draw will fall to his opponent. Concerning the second question, it is clear that any number which is greater than 0, or than any other member of the basic primary C group, by exactly the amount of H, cannot be critical; because *it can be reduced by one draw to less than L,* i.e., to a basic critical number. This is the reason why 4, 6, and 8 are not critical numbers in Series 1 or 4, 1 or 6, and 1 or 8 respectively. This is the reason also why the secondary C groups sometimes consist of less than L critical numbers, as in Series 2 or 5, 2 or 9, 3 or 7, 3 or 8, and 4 or 9. Each of the numbers in these series, which would have been critical but for the limiting effect of the H draw, is indicated in the list by a dash. All numbers that are greater than a basic secondary critical number by an exact multiple of L + H, are themselves critical for the same reason that numbers which are greater than a basic primary critical number by that amount, are critical.

To recapitulate, we have found certain uniformities in the distribution of critical numbers in our series of problems as follows: (1) There is in *every* series a group of critical numbers extending from 0 to L minus 1 inclusive, which we have called the basic primary C group. (2) All numbers in any series, which are greater than any number in the basic primary C group by exactly a multiple of the sum of L + H, are critical. (3) In the discontinuous series all numbers below H which are greater than any number in the basic primary C group by exactly a multiple of 2L, are critical. These are the basic secondary critical numbers. (4) All numbers in a series, which are greater than a basic secondary critical number by exactly a multiple of the sum of L + H, are critical.[6] The primary critical numbers are found in all series, both continuous and discontinuous; secondary critical numbers are found only in discontinuous series. To win a game, or trial, beginning with any number which is not critical, one must draw so as to reduce the number of beads to a critical number at his first draw; thereafter he must draw so as to make the sum of each of his draws and the preceding draw of the opponent, equal to L + H.[7]

---

[6] Other more difficult series of problems may be obtained by permitting three or more non-consecutive numbers or groups of numbers to be drawn. But the foregoing series furnish enough difficulty for the ordinary graduate student under the conditions of our experiment.

[7] In the discontinuous series this procedure may result in the final re-

B. Apparatus and Recording of Data

The apparatus consisted of a stop-watch, a metronome, an Edison telescribe, or dictaphone, and a row of thirty beads strung upon a steel wire the ends of which were inserted into metal cubes of sufficient height to permit the beads to move freely. These beads were substituted for the matches of the original game because they offered less obstruction to free manipulation, and therefore increased the value of the time records of individual reactions.

While the experiment progressed the dictaphone was in action with the horn so adjusted as to secure as clear a record as possible of all the verbal reactions of the subject. The subject was asked to state aloud the number taken at each draw as the "move" was being made. The experimenter also called out the number taken by him at each draw. Thus the dictaphone records contained all the draws and such comments as were made during the experiment. To mark the time consumed by these reactions a metronome beating half seconds was placed near the dictaphone. The beats were clearly audible in the records. As the experiment progressed the experimenter made a written record of the individual draws of the subject and of the experimenter in separate columns. Incipient movements, indicating that the subject was considering a particular draw, were recorded with a distinctive mark but were not counted in the summing up of data. Upon these written records the dictaphone records were later transcribed. The beats of the metronome were counted, and the number of half seconds from the beginning of the trial to each of its component reactions were recorded. Though no high degree of accuracy is claimed for these time records, they were accurate enough to be exceedingly useful in showing the distribution of attention among the different reactions entering into a trial. They also serve to show differences in the speed of reaction of different subjects and of any one subject in different portions of the experiment. From the dictaphone records were

---

duction of the number of beads to a basic secondary critical number, which will be reduced to a basic primary critical number by a series of L draws.

also obtained the comments of subjects, which were recorded in their proper places in the records. These verbal reactions were often useful in the interpretation of our data. In order that the learning process might go on as naturally and as free from interruption as possible, no introspections were called for and few were given.

For various reasons it was found necessary in certain portions of the work to dispense with the use of the dictaphone except for the recording of the more important verbal responses. Time measurements were here obtained by means of a stop-watch. In such cases it was of course impossible to record the time of each separate reaction, although the time of complete trials was taken with as great accuracy as was possible by the more cumbersome method. With this procedure it was not possible to distinguish the time consumed by the experimenter's draws from that consumed by the reactions of the subject. To make the records of different subjects comparable in time, therefore, the experimenter made the time of his draws as uniform as possible. A fair estimate of the degree of their uniformity may be made from the record of a relatively small number of typical reactions. In 206 trials with 11 beads taken from the records of various subjects, it was found that the average time per trial was 41.14 metronome beats, or half seconds. The average time per trial consumed by the experimenter's draws was 9.76 half seconds with an average deviation of 1.99. This A.D. of 20 per cent from the average of the experimenter's time per trial is not a serious matter when it is remembered that his time was on the average less than one-fourth of that consumed by the reactions of the subject. The effect of the inclusion of the time required for the experimenter's draws is to put the subject who draws most rapidly at a slight disadvantage, but the time differences in our records are large enough to make these small errors rather unimportant.

C. ORDER OF PRESENTATION OF PROBLEMS

As already stated, each series consists of what are to the subject at the beginning a succession of relatively independent problems. Beginning with H + 2 beads in all series in which L is 1, and with H + 2L — 1 beads in all series in which L is greater than 1, the numbers (or problems) of each series were presented in the order of their magnitude from lowest to highest. As soon as a practicable solution was found for one number, the next higher number of the series was presented. Thus successive problems of a series were solved until a satisfactory generalization was formulated for the entire series. A higher series of problems was then presented in the same manner. The order of presentation of series was the order of the magnitude of their L and H draws. First, all series in which L is 1 were presented, beginning with Series 1-2 and proceeding through Series 1-3, 1-4, 1-5, etc., until a general solution for all series of this *order*[8] was found. Series of the next higher order (i.e., in which L is 2) were next presented, beginning with Series 2-3 and continuing through Series 2-4, 2-5, etc., until a general solution for all series in which L is 2, was found. In the same manner successively higher orders of series were presented until a general solution was obtained for all continuous series. The number of continuous series was not the same for all subjects. A workable generalization was made by some subjects after the solution of but a few series in only two or three orders. Others were unable to arrive at a suitable generalization until after the solution of numerous series extending through a number of orders. Only a limited number of discontinuous series was presented, however, and this number was the same for all subjects. These discontinuous series are listed on page 7 in the order in which they were presented.

The order of presentation of the discontinuous series may need a word of explanation. The first portion of this group of series, extending from Series 1 or 3 to Series 4 or 12 inclusive, was given to bring out the fact that secondary critical numbers are found by the addition of multiples of 2L to the primary critical numbers. This is the simplest possible collection of series with a sufficient variety of values for L and H, in which all multiples of 2L above any primary critical number, are critical. Not all subjects succeeded in getting this generalization from this small group of series, but the addition of other series of this character was impracticable owing to the large number of beads that would need to be manipulated. In Series 1 or 4 to 4 or 9 it was expected that subjects would discover that not all multiples of 2L above a primary critical number, are critical; and also find some rule by which to locate such multiples of 2L above primary critical numbers as are not, in a given series, critical.

---

[8] The *order* of a series refers to the number represented by L. In series of the first order L is 1, in the second order L is 2, etc.

After the completion of Series 4 or 9 the beads were put aside, and the subject was asked to name the critical numbers in Series 4 or 11 and later in Series 5 or 11. The work in these series was necessarily done mentally and often served to compel the subject to return to his earlier generalizations in the attempt to find a principle which would apply here. If at the end of these series no single workable generalization for all series had been given, the subject was permitted to refer to a table of critical numbers for all the discontinuous series.[9] Here, as in the preceding portions of the work the time required and such of the subject's observations as were given verbal expression, were recorded.

D. Degree of Learning and Uniformity of Procedure

In order to make the conditions uniform for all subjects it was necessary that the experimenter govern his draws in accordance with some definite rules. It was, of course, necessary that no rule should prevent the experimenter from taking advantage immediately of any error made by the subject, but aside from this there was a fair latitude within which he might vary his draws without necessarily affecting the outcome of the game. In Series 1-2, for example, so long as the subject took care to reduce the number of beads to a multiple of 3 at each of his draws, it might seem to be a matter of indifference whether the experimenter took 1 or 2 at a draw. But the indiscriminate changing of his draws would result in a serious lack of uniformity in the condition to be met by the different subjects. If, on the other hand, the experimenter were to take 1 always or 2 always so long as the subject's draws were correct the latter might, by mere mechanical memory and repetition of previous draws, win a sufficient number of consecutive trials to permit him to pass on to the next higher number in the series, and some mechanical performance of this sort might be repeated indefinitely. To prevent such mechanical repetition of accidental successes while still maintaining uniform conditions for all subjects, the experimenter first drew 1 at every draw through an entire trial and then 2 at every draw throughout the next trial. This change in the number drawn by the experimenter in alternate trials was continued until the problem was solved. But if S made an error in any trial, E immediately departed from his uniform reactions and, taking advantage of the error, drew so as to win. The change in E's draw in alternate trials often proved very disconcerting for a time to subjects who tried to repeat accidental successes from memory, but it usually resulted in bringing about some kind of attempt at critical analysis. When two trials had been won in succession by the subject, another bead was added and the process repeated. Thus successive increments of 1 were made and the solutions for the resulting numbers found until a solution for the entire series was obtained.

---

[9] This table with a few changes in the italics is given on page 7.

This changing back and forth of the experimenter's draw in alternate trials, together with the requirement that S win two trials in succession, resulted in bringing out all possible variations in Series 1-2. Two lines of procedure each somewhat in accord with this were possible in the higher series. (1) E might alternate the L draw in one trial with the H draw in the next, neglecting all the intermediate draws except when an error on the part of the subject made it possible for E to win with one of the intermediate draws. (2) He might use *all* of the possible draws, taking each exclusively for an entire trial in some regular order. The latter procedure was used with some of the subjects in the preliminary experiments. It has the advantage of preventing, in a measure, the direction of attention to non-essentials. But, if consistently carried out, it requires that the subject win each number in the series as many times as there are possible draws in the series and so interferes seriously with the comparability of data from different series. The requirement that S win H minus L consecutive trials with each number also becomes somewhat monotonous in the higher series. The former procedure has the advantage of avoiding this monotony and keeping the degree of learning more nearly uniform throughout all series. It therefore makes the data from different series more truly comparable. This procedure was followed in the work of all of our subjects except a few who served in the preliminary experiments. No attempt is made to compare their work in the later series with that of other subjects.[10]

As soon as a subject recognized two successive critical numbers in a series by inspection, or showed his mastery by prediction of the outcome with higher numbers in the series, or gave a correct generalization, he was presented with a considerably higher number in the same series. He did not draw from this higher number, but merely announced his decision as to what would be the correct draw and gave a reason for his decision. In case the correct draw was indicated but no intelligible reason could be given, the subject was taken back to the point previously attained and required to follow the regular procedure until clear evidence was given of at least some sort of a practical solution which would hold for all numbers in the series. This was not a very common occurrence, however, and never resulted in serious departures from the ordinary procedure. In most cases a verbal formula was given, and sometimes this was expressed in mathematical form.

---

[10] Even this procedure cannot, however, be said to make the data from different series entirely comparable. The fact that the critical and the non-critical numbers are not in the same proportion in different series, is bound to affect the degree of learning in some measure. Compare Series 1-2 with Series 1-5 in this respect. In the former series the subject encounters five critical numbers in working from the beginning of the series to and including 15 beads; in the latter he encounters two. In the former series he need not reduce the number of beads to each of these critical numbers more than four times before passing to the next higher critical number. In the latter series he must reduce the beads to each of the critical numbers at least ten times before he is permitted to pass on to the next higher critical number. There is, of course, in the more frequent occurrence of critical numbers in the lower series, some compensation for the higher degree of learning of critical numbers in the higher series.

At the completion of a series the next higher series followed immediately or at the next sitting. The solving of successive series continued until a general solution for an entire order of series was given or until a complete series was solved without a draw. When this occurred, a higher "test" series of the same order was usually but not always given, being omitted only when the subject's statement of his generalization left no doubt that he had found an adequate solution for all series of that order. In case of failure in one of these test series, it need hardly be said, a return was made to the series following the last one which had been solved. If the test series was successfullly solved, the first series of a higher order was presented, and so on throughout the experiment. As already stated, it was impossible to adhere strictly to this uniform degree of learning in case of the discontinuous series. Here the degree of learning differed considerably with different subjects, some of whom persistently worked out the relations existing between successive series whereas others were content to generalize for each series in isolation.

E. INSTRUCTIONS TO SUBJECTS

With the apparatus in position and the subject ready to begin work, the experimenter said in substance: "We shall draw alternately from this string of beads. You may take 1 or 2 beads at a draw and I also may take 1 or 2 at any draw. (With each statement of the number to be drawn the experimenter illustrated by manipulation of the beads, showing the alternation between his and the subject's draws which were made from opposite ends of the row of beads.) The object is to get the last draw, i.e., you win if you get the last draw. You always draw first. Will you call out the number you take at each draw?"

The number was then immediately reduced to 4 and the subject asked to begin. The procedure from this point has already been described. As soon as the subject announced his inability to win with 6 beads as the initial number, the experimenter asked: "Hereafter whenever you make such a discovery or get an idea which seems to be significant, will you let me know at once?" The subject was also told at this point, provided his questions had not brought out the fact earlier, that he would be expected to find a general rule for the solution of all numbers in the series. The request that S make known any seemingly relevant new ideas was repeated at the beginning of each of the early series and at other times when his actions seemed to indicate that he had made a new discovery. Subjects were not permitted to record any data though the experimenter's record for a few of the immediately preceding trials was usually in sight. This was unavoidable owing to the difficulties of manipulation and recording. While attempting to recall successful reactions of earlier portions of the work, subjects sometimes asked permission to see their records. This was not permitted. At the beginning of the second series the subject was told: "The requirements of the game are the same as before but you may now take either 1, 2, or 3 beads at a draw." The subjects were of course informed at the beginning of each of the later series as to the numbers which might be drawn. Subjects were asked to refrain from thinking of the experiments during the intervals between their periods of work.

F. THE SUBJECTS

Exclusive of those who served in the preliminary experimentation by which the final mode of procedure was determined, 46 subjects served in our experiments. The subjects were divided into groups as follows: (a) Group I consisting of 14 subjects, (b) Group II consisting of 20 subjects, (c) Group III consisting of 12 subjects. The major portion of our study is based upon the work of the subjects in Group I. These subjects will be designated by Roman numerals. Subjects i to x are numbered in the order of the speed of their performance as determined by the number of trials required for the solution of all the problems presented, Subject i being the speediest, Subject ii the next speediest, etc. Subjects xi, xii, xiii, and xiv did not finish the entire experiment and could not, therefore, be given their relative ranks. Subject iii was a first-year high school boy who was thirteen years old; Subject vi was a college senior, and the remaining ones were all graduate students or instructors in the University of Chicago. All of the subjects of this group were males except Subject v.

Subject ii solved most of the problems at two sittings in one day and returned three weeks later to finish in one short period. Subjects i, iii, iv, vi, vii, and xii were scheduled to work every day, but failed to live completely up to the schedule. Subjects ix and x worked with a fair degree of regularity at weekly intervals, and the remaining subjects of this group were quite irregular in the distribution of their effort. The work with this group of subjects was done at the University of Chicago during the fall and winter of 1916-17.

Group II consists of 20 members of a class of 21 third- and fourth-year college and graduate students at the Kansas State Agricultural College. This group is subdivided into Groups IIa and IIb each consisting of 10 subjects. Group IIa solved first Series 1-2 and then Series 1-3. The members of this group are designated by the capital letters A to J in the order of their speed of learning, the speediest first. Group IIb solved first Series 1-3 and then Series 1-2. The members of this group are designated by the small letters from a to j, again in the order of their speed of learning. The work with Group II was done at the Kansas State Agricultural College in the spring of 1918.

Group III consists of all of the members of a class of third- and fourth-year college students. The subjects in this group worked out solutions for problems in Series 1-2 from 14 to 20 inclusive. The work with this group of subjects was done at the Kansas State Agricultural College during the fall of 1918.

III. RESULTS

A. UNIT OF MEASUREMENT

Before proceeding with the presentation of data it is necessary to attend to the selection of a suitable unit for the measurement of progress. To be entirely comparable successive units of effort should be uniform in (a) time consumed, (b) degree and distribution of attention, and (c) number and character of reactions. Perfect uniformity in all of these characteristics is obviously out of the question, if subjects are to be given the requisite freedom for normal reasoning. There is no constant unit of time into which the work of our subjects can be divided so that the distribution of effort in successive units will conform even approximately to (b) and (c) of the foregoing requirements. Of a number of units of response which suggest themselves the trial[1] conforms most closely to all of these requirements.[2] Measurement and comparison of progress will therefore be made chiefly in terms of the trial, though the number of errors made during the solution of the various problems, as well as the time required, will also be stated and utilized to some extent in the treatment of some portions of our data.

It will be worth while briefly to inquire into the variability of the trial. The characteristics in which variability is greatest and also most susceptible to accurate measurement, are the time and number of draws per trial. The facts regarding the variability of trials in these characteristics, as found in the work of our major group of subjects in Series 1-2, are given in Table I.

---

[1] A series of draws culminating in the reduction of any initial number of beads to 0, constitutes a trial, regardless of who draws last.

[2] It might be supposed that the *draw* would be a better unit of measurement, but this unit is altogether too variable to be of service. In time consumed it varies from less than a half second to several hundred seconds. In the amount of critical attention called forth, it varies from serious, concentrated effort to the most mechanical performance. Often, indeed, the draws do not stand out singly as significant to the subject, but are linked up in various combinations into series each of which constitutes a trial and a real unit in the distribution of attention. Nor can the measurement of progress be made in terms of errors or of successful performances,—i.e., of successes in reducing the number of beads to 0,—since in drawing from a critical number no successes of this objective sort and likewise no erroneous draws, are possible.

TABLE I

	No. of Subject's Draws per Trial			Time per Trial		
Subj.	Av. No. Draws	A.D.	V[3]	Med. Time	Q[4]	V[5]
iii	3.13	.95	.30	27	7.0	.26
iv	3.33	.71	.21	39	13.1	.34
i	3.35	.67	.20	41	11.7	.29
v	3.58	.91	.25	58	30.0	.52
ii	3.66	.89	.24	85	34.9	.41
viii	4.20	.68	.16	25	5.4	.22
vii	4.21	1.41	.34	27	10.5	.39
xi	4.25	1.02	.24	54	16.2	.30
ix	4.31	1.29	.29	26	6.8	.26
xiii	5.01	1.68	.33	25	9.5	.38
xii	5.22	1.62	.31	48	26.5	.55
x	6.84	2.89	.42	43	14.6	.33
vi	7.15	1.68	.23	36	10.3	.29
xiv	8.03	3.64	.45	33	14.5	.44

[3] $V = \frac{\text{A.D.}}{\text{Av.}}$ [4] $Q = \frac{Q^3 - Q^1}{2}$ [5] $V = \frac{Q}{\text{Med.}}$

The table is self-explanatory. Note the slight tendency for subjects who rank high in the number of draws per trial to rank low in time per trial. The average of the average numbers of draws per trial for the first seven subjects listed is 3.64 and the average of their median times per trial is 21.5 seconds. The corresponding averages for the last seven subjects listed are 5.83 draws and 16.9 seconds. The correlation between these two lists of averages is —.105.[6] The variations in time and in number of draws per trial do not, therefore, tend to show any cumulative effect upon the relative values of trials in the work of different subjects. Moreover, the distribution of attention is such as largely to discount the value of variations in time and number of draws per trial—especially the latter. It seldom occurred that a subject attended carefully to all of the draws in a trial when the initial number of beads was higher than 17 or 18. One portion of the trial was usually regarded as crucial and the attention mainly directed upon that point with the result that all but a few of the draws in the trial were executed with only a minimum of attention. If these mechanical draws were here neglected, the variation in number of draws per trial would be extremely slight, and the time variations would actually tend to compensate for the variations in the number of draws per trial.[7] Again, after the longer pauses, subjects frequently came back to the work declaring that they had permitted their minds to wander in the pursuit of irrelevant associations. The comments of subjects indicate that such distractions occurred rather frequently. If these distractions are to be regarded as indications of a low degree of attention, we have here another case of an inverse or compensating relationship between variations in different aspects of the trial. Such practice value as the mechanical draws may have possessed was perhaps completely offset by their distracting effect upon attention to points which were regarded as crucial. At any rate this was the spontaneous verdict of a number of the subjects.

---

[6] Rank method.

The variability of trials within Series 1-2 is typical of that in all later series in which any considerable difficulty was encountered. There was no great variation in the average length and duration of trials from series to series. In the later series—especially those which were presented late in any group of series of the same order—there were generally fewer draws per trial, and attention tended to be more evenly distributed among the various draws of the trial, though mechanical draws practically always appeared in any series where the number of draws per trial ran high. The average time per trial required by Subjects i to x inclusive in some of the more difficult series, is given in Table II. The series reported in this table represent approximately two-thirds of all trials of these subjects in the entire experiment.

[7] This compensating relation in the variations of time and number of draws per trial is not, of course, found in the trials upon higher as compared with those upon lower numbers of a series in the work of the same subject. Here the time per trial usually varies directly with the number of draws though not in the same proportion. But even here it is doubtful whether the longer trials are of more value than the shorter ones, owing to the presence and distracting effect of the more mechanical draws.

TABLE II

Series	No. Trials	Av. Time per Trial[8] (in seconds)	A.D.	V.
1-2	2060	24.09	5.46	.23
2-3	647	42.72	10.68	.25
2 or 6	360	43.15	23.69	.55
1 or 4	423	38.42	14.11	.37
2 or 7	265	42.19	2.94	.07

[8] These averages and deviations are those of all trials in a series without regard to the average time per trial of individual subjects.

There is a general tendency for subjects to exercise a little more caution after the solution of one or two series than at first. This accounts for the marked increase in the average time per trial in Series 2-3 as compared with that of Series 1-2. In the last four series listed the uniformity of the average time per trial is fair, but the deviations are large. However, it should not be overlooked that in the more important matter of degree and distribution of attention the variability, though not susceptible to quantitative statement, is undoubtedly much less marked, and that the variations in one aspect of a trial often tend to compensate for these in other aspects. On the whole, it is safe to say, the trial in the present experiment is not less uniform than the units of measurement regularly employed in studies on maze learning. Its uniformity is probably far greater than that of the trials in certain experiments in ball-tossing, from which curves of learning have been plotted and conclusions of far-reaching consequence drawn.

B. CHANGES IN THE RATE AND CHARACTER OF PROGRESS

The progress of the ten subjects of our major group, who solved all of the series of problems, can be traced in Table III. The series of problems are listed at the left in the first column. Following each series are the records of the work of individual subjects upon it. In columns P and E are given the number of problems (i.e. of different numbers within a series) which are solved and the number of errors made in arriving at a general solution of the series. The time is given in seconds.

TABLE III

Progress of Subjects through all Series of Problems

	Subject i				Subject ii[9]				Subject iii			
Series	P	E	Trials	Time	P	E	Trials	Time	P	E	Trials	Time
1-2	10	24	48	1716	12	8	35	1572	12	5	28	464
1-3	4	0	7	247	9	4	23	585	5	1	10	120
1-4	5	5	14	124	0	0	0	0	0	0	0	0
1-5	0	0	0	19	0	0	0	0	0	0	0	0
2-3	22	11	44	1661	8	3	17	507	11	1	25	427
2-4	0	0	0	10	4	0	4	215	28	13	84	1685
2-5	0	0	0	0	0	0	0	38	16	13	60	1115
2-6	0	0	0	0	0	0	0	0	0	0	0	32
3-4	0	0	0	165	0	0	0	53	0	0	0	15
3-5	0	0	0	123	0	0	0	0	0	0	0	0
4-5	0	0	0	215	0	0	0	50	0	0	0	45
1 or 3	4	—	4	667	8	—	8	269	7	—	11	115
1 or 5	12	—	13	1387	2	—	3	118	6	—	13	173
1 or 7	0	—	0	54	0	—	0	36	4	—	5	62
2 or 6	17	—	21	2483	3	—	3	178	15	—	30	618
2 or 10	0	—	0	483	4	—	4	201	12	—	18	289
3 or 9	7	—	6	381	5	—	6	437	0	—	0	65
4 or 12	0	—	0	120	0	—	0	193	0	—	0	14
1 or 4	4	1	8	492	12	15	38	2701	20	6	47	757
1 or 6	0	0	0	46	24	1	47	4076	14	1	29	467
1 or 8	0	0	0	23	11	1	19	636	0	0	0	193
2 or 5	0	0	0	34	11	0	14	606	6	0	9	361
2 or 7	0	0	0	32	12	0	15	644	16	1	19	835
2 or 8	0	0	0	342					0	0	0	164
2 or 9	0	0	0	78	11	0	15	647	0	0	0	55
3 or 7	0	0	0	35					0	0	0	80
3 or 8	0	0	0	21	11	0	15	664	0	0	0	27
4 or 9	0	0	0	38	0	0	0	310	0	0	0	90
4 or 11				40				365				25
5 or 11				30				230				62
General solution for all series				155				533				70
Total	85	41	165	11215	147	32	266	15864	172	41	388	8425

---

[9] Series 2 or 8 and 3 or 7 were accidentally left out of the experiment with this subject.

TABLE III (Continued)

	Subject iv				Subject v				Subject vi			
Series	P	E	Trials	Time	P	E	Trials	Time	P	E	Trials	Time
1-2	11	55	96	2205	13	23	62	2834	27	221	331	7934
1-3	12	13	35	704	8	1	15	363	11	6	30	265
1-4	10	6	27	449	2	0	3	33	2	0	4	54
1-5	6	0	10	87	0	0	0	9	0	0	0	40
1-6	14	0	24	241	0	0	0	0	0	0	0	0
1-7	8	0	14	157	0	0	0	0	0	0	0	0
1-8	0	0	0	36	0	0	0	0	0	0	0	0
2-3	13	16	44	1800	21	19	71	4566	15	5	30	709
2-4	6	1	13	279	13	7	29	894	1	0	1	268
2-5	0	0	0	30	0	0	0	54	0	0	0	0
3-4	0	0	0	25	0	0	0	17	1	0	0	205
3-5	0	0	0	0	0	0	0	28	0	0	0	74
4-5	0	0	0	26	0	0	0	10	0	0	0	75
1 or 3	7	—	17	180	4	—	4	52	10	—	10	237
1 or 5	5	—	9	110	10	—	18	377	14	—	18	503
1 or 7	2	—	3	33	10	—	14	269	13	—	14	235
2 or 6	27	—	45	1818	33	—	46	1189	24	—	36	821
2 or 10	6	—	11	546	25	—	32	916	11	—	14	262
3 or 9	4	—	6	151	0	—	0	72	13	—	13	258
4 or 12	0	—	0	75	0	—	0	165	9	—	8	142
1 or 4	12	7	26	430	13	3	21	392	19	1	31	484
1 or 6	13	5	24	1006	23	5	47	1657	6	0	8	147
1 or 8	0	0	0	58	16	5	28	984	2	0	2	66
2 or 5	0	0	0	152	8	0	8	427	16	2	21	346
2 or 7	8	0	14	590	21	2	34	1631	6	0	6	145
2 or 8	2	0	2	333	7	0	10	527	3	0	4	75
2 or 9	0	0	0	58	10	0	15	993	0	0	0	66
3 or 7	17	6	33	1526	0	0	0	285	8	0	10	159
3 or 8	0	0	0	600	0	0	0	192	3	0	2	58
4 or 9	0	0	0	604	0	0	0	40	0	0	0	165
4 or 11				820				85				65
5 or 11				55				290				35
General solution for all series				8610				1210*				280
Total	183	109	453	23794	227	65	457	20561	214	235	594	14173

TABLE III (Continued)

	Subject vii				Subject viii				Subject ix			
Series	P	E	Trials	Time	P	E	Trials	Time	P	E	Trials	Time
1-2	19	182	371	6659	15	90	168	2591	28	187	315	6129
1-3	12	3	23	754	12	24	56	684	6	4	16	244
1-4	5	0	8	207	10	8	30	294	2	0	3	23
1-5	0	0	0	15	1	0	2	23	1	0	2	52
1-6	0	0	0	0	0	0	0	15	4	0	9	115
1-7	0	0	0	0	0	0	0	0	0	0	0	6
2-3	18	1	25	1827	69	27	167	5408	42	25	119	5859
2-4	9	0	13	1047	35	9	71	2679	25	4	53	1956
2-5	0	0	0	28	15	0	20	500	10	1	15	295
2-6	0	0	0	0	2	0	2	59	0	0	0	35
2-7	0	0	0	0	0	0	0	9	0	0	0	0
3-4	8	0	12	301	19	4	41	699	9	0	10	155
3-5	0	0	0	63	3	0	4	75	6	0	9	118
3-6	19	1	30	3115	0	0	0	13	0	0	0	20
3-7	0	0	0	21	0	0	0	0	0	0	0	0
4-5	0	0	0	12	0	0	0	21	0	0	0	0
1 or 3	6	—	12	372	24	—	33	1067	17	—	36	1101
1 or 5	8	—	11	327	19	—	24	870	5	—	11	195
1 or 7	0	—	0	6	3	—	4	76	1	—	2	52
2 or 6	19	—	19	2427	23	—	38	1268	51	—	108	4189
2 or 10	7	—	16	979	29	—	34	755	17	—	20	489
3 or 9	0	—	0	60	19	—	20	309	14	—	18	680
4 or 12	0	—	0	80	0	—	0	52	0	—	0	87
1 or 4	14	1	22	1583	24	2	39	877	37	41	154	7230
1 or 6	12	1	19	4691	12	0	16	303	14	0	24	1318
1 or 8	0	0	0	30	0	0	0	40	14	0	21	947
2 or 5	11	2	20	728	24	8	46	1441	11	1	19	758
2 or 7	7	0	12	614	48	12	86	3702	12	3	27	966
2 or 8	7	0	13	954	9	2	14	263	9	2	13	446
2 or 9	8	0	10	1102	12	1	15	271	10	0	15	660
3 or 7	7	2	13	293	26	2	34	677	10	0	21	360
3 or 8	8	0	8	678	11	0	14	369	10	0	17	369
4 or 9	4	0	5	828	8	0	9	278	11	1	22	589
4 or 11				455				690				240
5 or 11				1105				910				150
General solution for all series				4980				2420				7740*
Total	208	193	662	36341	472	189	987	29708	376	269	1970	43753

TABLE III (Continued)

Subject x

Series	P	E	Trials	Time	Series	P	E	Trials	Time
1-2	47	313	597	17538	2 or 10	7	—	7	233
1-3	16	3	45	1122	3 or 9	27	—	28	980
1-4	2	0	0	136	4 or 12	9	—	8	514
1-5	0	0	0	0	1 or 4	19	11	37	1306
2-3	36	16	105	4881	1 or 6	22	14	49	1421
2-4	15	0	26	728	1 or 8	26	5	36	1973
2-5	3	0	2	77	2 or 5	14	3	21	1016
2-6	0	0	0	0	2 or 7	35	4	50	1922
3-4	37	0	40	240	2 or 8	9	0	12	377
3-5	1	0	0	230	2 or 9	1	0	0	118
3-6	10	0	12	444	3 or 7	19	1	21	968
317	0	0	0	28	3 or 8	12	1	14	548
4-5	11	0	10	570	4 or 9	12	2	18	485
5-6	0	0	0	50	4 or 11[11]				327
1 or 3	21	—[10]	23	858	5 or 11				193
1 or 5	8	—	10	466	General solution				
1 or 7	3	—	4	225	for all series				1960*
2 or 6	13	—	14	545	Totals	435	373	1192	42470

The most striking feature of these figures is the great irregularity in the number of trials, the time required, etc., for the solution of various problems. Note the relatively large number of trials required in Series 1-2, and the rapid decrease in the number of trials in successive series of the first order.[12] The same sort of change is found in series of the second order and, to some extent, in all higher orders of the continuous series as well as in those of the discontinuous series. These regular and more or less gradual changes are due in part to the separate mastery of numerous relatively isolated though often poorly defined "elements" of the problematic situation; in part they are due to a gradual definition and development of larger and more complex units in the form of concepts and general principles.[13]

---

[10] No errors are possible in those series where blanks appear in the error column.

[11] Subjects were required to work Series 4 or 11 and 5 or 11 "mentally."

* These subjects failed to find a general solution for all series in the time at their disposal.

[12] The *order* to which a series belongs is determined by the value of L. In series of the first order L is 1; in those of the second order L is 2, etc.

[13] Any aspect or isolable portion of the objective situation and any relationship between such portions or aspects is here regarded as an element of the problem.

These gradual changes are sometimes obscured by irregular and often extremely abrupt changes arising from various causes, such as, distraction of attention, sudden utilization of old concepts, monotony, fatigue, rest, elimination of erroneous assumptions, change of methods, etc.

The more striking irregularities in the rate of progress are shown in graphic form in Figure I. The number of trials and the amount of time required as well as the number of errors made by each of the ten subjects in various series and groups of series, is represented by the columns in the figure. The series and groups of series represented in the figure are indicated at the left. The subjects are designated by the numerals at the base of columns.

This irregularity is in some degree illusory owing to the failure of some bonds in the early stages of their development to register their effect in terms of those particular overt responses which were taken into account in the score. That bonds do begin to develop long before their effect is apparent in the crude score, is evident from the comment of subjects, the comparative length of delays, incipient reactions, etc., some of which are included in the detailed records and will be noted later. Further evidence of the inadequacy of any one sort of measure to register all of the changes in the strength of bonds, is found in the relation between the time, the errors, and the number of trials involved in the solution of successive series of problems. In Figure I the number of errors made and the amount of time consumed in the solution of the problems in each section of the figure are shown by the relative height of the first and third columns respectively. Observe that in every case in Series 1-2 the error column is higher than the time column, and that there is a somewhat gradual shifting in the relative height of these columns so that in the last group of series the time column is in every case higher than the error column.[14] This change is to

---

[14] The relation between the trial column and the time and error columns in Series 1-2 and in the last group of series, is worthy of note. In every case but one the height of the trial column is intermediate between that of the other two columns. The measurement of progress in terms of trials therefore takes special account of the conflicting claims of time and errors where the conflict is greatest.

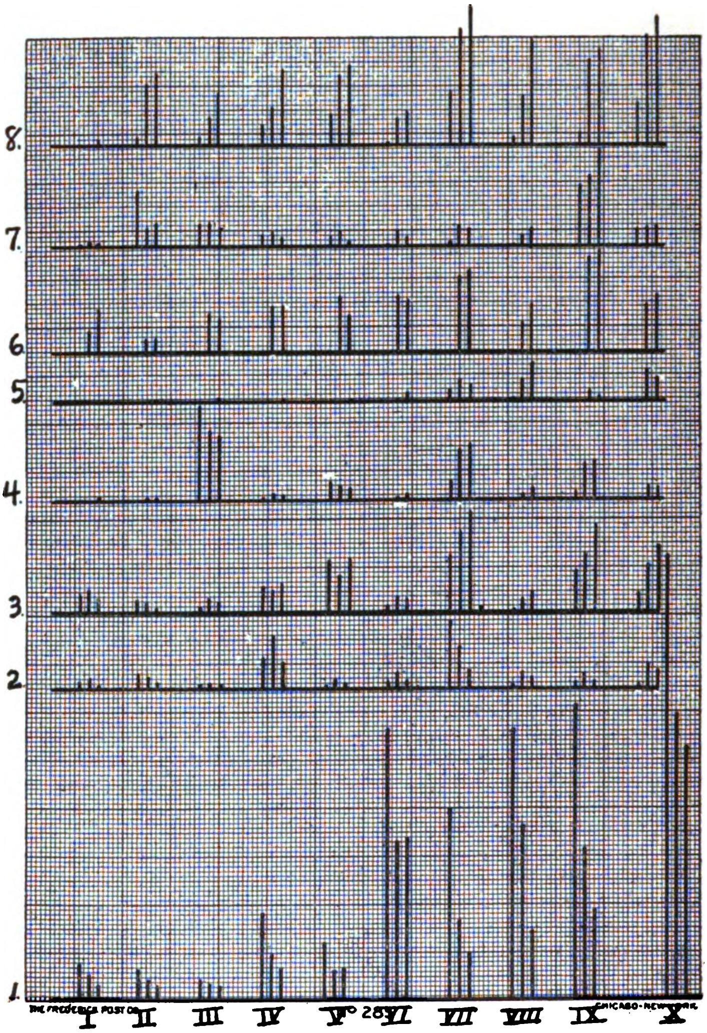


Figure I.—The Roman numerals at the base of the columns of the lowest section represent the various subjects of Group I, who solved all series. The height of the middle column for each subject represents the number of trials required by him for the solution of the problems indicated by the number at the left. The *trial columns* are all directly comparable. The number of errors made and the amount of time required by each subject are represented by his first and third columns respectively. The time and error columns are drawn to such a scale for each subject as to make their total length for all series equal to the total length of the trial columns of that

some extent influenced by increased caution, as is shown by the lengthened average time per trial in the later series as compared with the first series; but it is due mainly to the fact that performance represents a distinctly lower plane of learning here than does formulation. It is a little surprising to find this relation between power of performance and power of formulation here in view of the simplicity and definiteness of the terms which are required for formulation. It is possible that if success were to demand a high speed of reaction, as is generally the case in acts of skill and to a large extent in practical thinking, this relationship would tend to disappear. However, with the time allowed for each reaction limited only by his patience, the subject is usually able to respond correctly while he is yet unable to foretell the correct response in the absence of the appropriate concrete situation or to say why the response is correct when made. This disparity in power of performance and power of formulation as well as some of the more striking cases of change in speed of progress, both of the regular and of the irregular sort, will receive more special attention in later sections.

The principal features of the progress from series to series are duplicated in the progress from problem to problem in some of the more novel series. There are, however, some important differances. Series 1-2 being the first and most novel series illustrates the natural progress from problem to problem better than do any of the later ones. The progress of the fourteen subjects of the major group through the problems of this series,

---

particular subject. They are thus directly comparable with one another and with the trial columns of the same subject. But the error and time columns of *different* subjects are *not* directly comparable.

The numbers at the left represent various series and groups of series of problems as follows:

1. Series 1-2.
2. All later continuous series in which L is 1.
3. Series 2-3.
4. All later continuous series in which L is 2.
5. All continuous series in which L is 3 or greater.
6. All discontinuous series from Series 1 or 3 to Series 4 or 12 inclusive
7. Series 1 or 4.
8. All remaining discontinuous series.

is shown in Table IV. The problems of the series are listed at the left in the column headed "Initial No. of Beads." The number of trials and of seconds required by each subject for the solution of each problem are listed in self-explanatory form.

The errors are not included in this table but their number is roughly proportional to the number of trials per problem except in the critical numbers, where no errors are possible. Certain points of difficulty for various subjects are revealed by the large number of trials and the amount of time required for the solution of some of the problems. Such points of unusual difficulty are found in the record of Subject iv at 10 beads, in that of Subject i at 11 beads, and in that of Subject vi at 25 beads, etc. Sometimes the troublesome problems are so distributed as to suggest definite, regularly recurring types of difficulty. This is illustrated in the record of Subject vi where numbers which are equal to 1 plus any multiple of 3 offer greater difficulty than those which are equal to 2 plus a multiple of 3. Problems of the former type will be referred to as L-problems and those of the latter type as H-problems. It will be noticed that the L-problems from 7 to 22 inclusive required 72 trials for their solution whereas the H-problems from 8 to 23 inclusive required only 25 trials. The solution of 25 beads which is an L-problem, required but 6 trials. Only in two instances did this subject solve an L-problem with

TABLE IV.—PROGRESS OF SUBJECTS THROUGH SERIES 1-2

Initial No.	Subj. i		Subj. ii		Subj. iii		Subj. iv		Subj. v	
of Beads	Trials	Time	Trials	Time	Trials	Time	Trials	Time	Trials	Time
4	2	21	2	29	3	42	4	32	2	20
5	2	13	2	29	2	24	4	28	2	17
6	4	48	2	35	3	36	3	44	2	32
7	2	25	2	97	3	27	7	117	6	89
8	4	59	3	96	4	56	4	79	4	78
9	5	86	3	70	1	21	9	148	11	240
10	5	104	2	79	3	50	50	1151	7	256
11	18	719	6	375	3	81	4	152	11	577
12	4	102	3	112	2	42	0	15	2	117
13	4	539	2	143	2	32	8	270	3	106
14	0	0	7	355	2	29	3	169	7	647
15	0	0	1	152	0	24	0	0	2	252
16	0	0	0	0	0	0	0	0	3	406
Total	48	1716	35	1572	28	464	96	2205	62	2834

Table IV (Continued)

Initial No.	Subj. x		Subj. ix		Subj. vi		Subj. vii		Subj. viii	
of Beads	Trials	Time	Trials	Time	Trials	Time	Trials	Time	Trials	Time
4	4	35	4	22	2	4	4	40	2	15
5	4	42	2	14	2	4	4	68	2	21
6	4	94	9	85	8	33	22	190	7	60
7	4	62	3	25	11	87	74	533	2	16
8	4	77	3	25	5	46	4	35	4	36
9	17	292	4	35	7	56	12	343	8	88
10	8	108	4	44	5	45	48	664	15	158
11	24	339	68	822	6	68	11	145	32	395
12	28	423	15	192	8	86	26	335	18	193
13	100	2067	6	94	15	211	12	156	33	559
14	9	204	76	1455	2	30	18	257	24	551
15	44	1229	3	54	7	101	36	590	12	264
16	48	1165	88	1799	10	151	11	237	3	104
17	48	1220	3	126	7	108	21	428	3	91
18	15	369	3	76	5	84	16	567	3	40
19	21	687	3	135	26	468	7	202	0	0
20	21	446	2	72	3	42	24	538	——	——
21	3	58	1	47	10	154	2	265	167	2591
22	38	1054	4	248	5	76	9	1064		
23	3	67	2	82	2	28	——	——		
24	6	154	2	34	7	123	371	6659		
25	5	121	2	137	156	5061				
26	3	78	2	84	6	126				
27	8	264	1	48	10	450				
28	2	68	2	114	4	100				
29	9	262	2	152	2	32				
30	5	141	0	19	0	157				
31	10	337	1	87	0	0				
32	6	244	0	0	——	——				
33	10	325	——	——	331	7934				
34	5	181	315	6129						
35	2	95								
36	5	187								
37	17	696								
38	2	77								
39	3	151								
40	7	284								

Subj. x (Continued)

No. of Beads	Trials	Time
41	2	100
42	2	114
43	7	480
44	3	237
45	0	1110
46	18	1212
47	2	151
48	0	19
49	6	467
50	4	400
	——	——
	597	17538

TABLE IV (Continued)

Initial No. of Beads	Subj. xiv Trials	Subj. xiv Time	Subj. xii Trials	Subj. xii Time	Subj. xiii Trials	Subj. xiii Time	Subj. xi Trials	Subj. xi Time
4	4	18	2	17	2	16	2	9
5	4	42	2	9	3	13	3	40
6	3	59	5	58	5	49	1	31
7	3	44	6	69	4	26	2	25
8	5	86	7	137	3	22	2	51
9	11	124	8	99	6	49	1	33
10	24	352	17	295	8	63	7	182
11	36	568	4	116	30	338	8	225
12	12	123	9	156	38	481	2	53
13	4	52	11	250	2	31	13	419
14	2	14	28	1016	4	89	2	53
15	10	131	7	256	6	103	1	8
16	5	93	39	1622	10	183	2	56
17	14	230	5	393	3	38	2	57
18	10	150	0	127	5	125	0	41
19	4	16	5	253	2	38	3	83
20	9	254	4	211	7	217	2	55
21	12	218	0	63	2	96	0	60
22	5	78	4	170	5	132	0	0
23	7	152	2	97	2	105	—	—
24	3	51	0	2	2	108	53	1481
25	13	295	3	114	4	132		
26	2	38	2	65	2	70		
27	4	68	0	1	2	216		
28	7	146	2	152	3	87		
29	2	37	2	58	2	76		
30	3	62	0	88	1	27		
31	14	355	2	91	3	136		
32	6	214	2	74	0	0		
33	2	68	0	22	—	—		
34	25	758	2	49	166	3066		
35	2	74	2	39				
36	2	72	0	5				
37	23	793	2	106				
38	5	117	0	0				
39	0	1	—	—				
40	7	373	184	6283				
41	3	134						
42	1	97						
43	18	678						
44	2	60						
45	0	4						
46	4	149						
47	4	148						
48	0	6						
49	4	116						

Subj. xiv (Continued)

No. Beads	Trials	Time
50	3	81
4	2	11
5	2	9
6	0	2
7	5	84
8	4	47
9	1	16
10	3	23
11	3	58
12	0	3
13	2	14
14	2	17
15	0	1
16	2	9
17	2	24
	—	—
	367	8171

fewer trials than were required for the preceding H-problem. A total of 236 trials was required for the solution of 9 L-problems and only 35 for 9 H-problems. Subject x required 296 trials for the solution of the L-problems of this series and only 146 for an equal number of H-problems. Subject xiv experienced a change in the type of difficulty at about the middle of the series. The L-problems from 7 to 22 inclusive required only 45 trials whereas the H-problems from 8 to 23 inclusive required 73 trials. But from this point to 44 beads the L-problems became the more difficult, requiring 107 trials as compared with the 22 trials required for the solution of an equal number of H-problems in this portion of the series. A similar change in the type of difficulty occurs in the record of subject vii. The L-problems below 12 required 126 trials while the H-problems were solved in 19 trials. Yet the numbers of trials required for the solution of an equal number of L- and H-problems above this point were 30 and 83 respectively.

The principal causes of these irregularities are found to be false assumptions; erroneous generalizations, founded usually upon very incomplete analysis; uncritical applications of methods and generalizations from preceding problems, and, in some of the extreme cases, monotony and fatigue. Some of these irregularities will be considered at greater length in later sections on generalization and transfer.

Another noteworthy feature of the progress of most of the subjects through the problems of Series 1-2 is the low number of trials upon the early and the late problems of the series as compared with the number of trials required for the solution of some of the intermediate problems. The extent to which this feature characterizes the work of various subjects is shown in Figure II. The number of trials is represented on the abscissae and the number of problems solved—or recognized as insoluble in case of the critical numbers—is represented on the ordinates. Note that with the exception of the curve of Subject iii, all curves show a period of relatively rapid advancement followed by one of slower progress, after which a final period of rapid progress terminates with a successful generalization for the entire series.

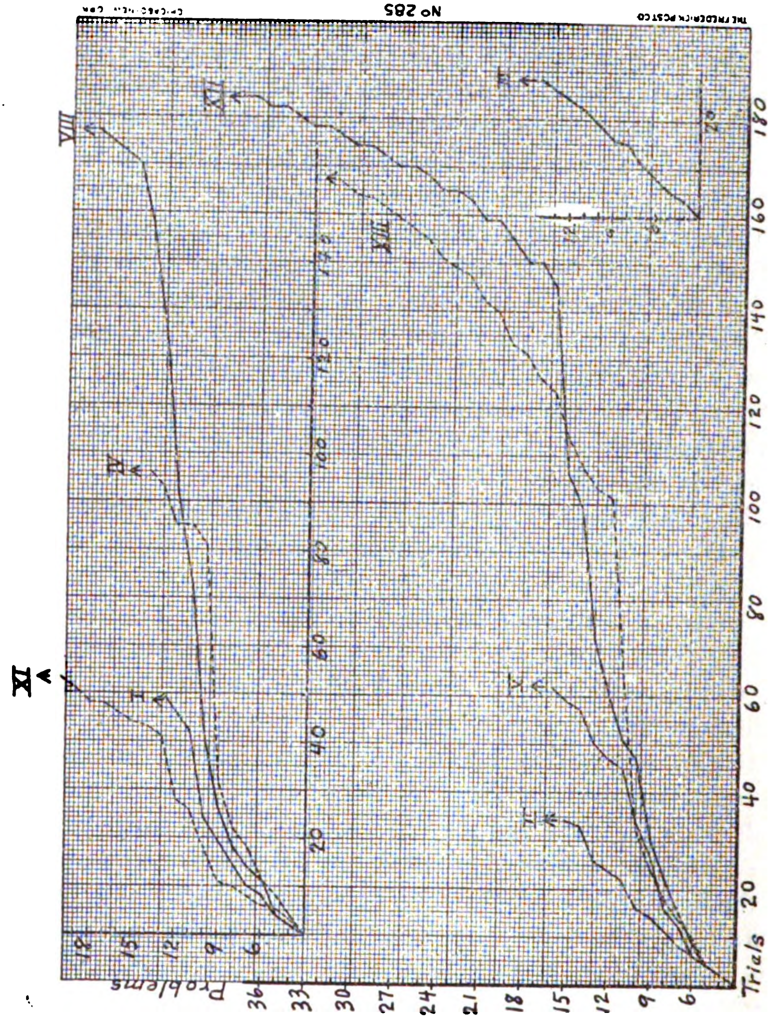

No 285
THE FREDERICK POST CO
CHICAGO
Problems
Trials
6
9
12
15
18
21
24
27
30
33
36
20
40
60
80
100
120
140
160
180
I
II
III
IV
V
VI
VII
VIII
IX
X
XI

FIGURE II.

The three stages in the learning process represented by these features of the curves are also clearly indicated by the behavior of the subjects and by their comments. The period of rapid initial rise of the curves was obviously a period of perceptual solutions. During the intermediate period of slower advancement the abstraction of significant elements of the problematic situation occupied the greater portion of the energies of the subjects. Some combination among these elements of course occurred immediately, but it was not until after considerable experience with the separate elements that most subjects were able to combine and organize them into effective means of control, capable of altering markedly the direction of the curve and leading speedily to a satisfactory generalization for the entire series. The types of mental process which in some degrees dominated these various stages of learning may profitably be treated separately in the three following sections.

C. PERCEPTUAL SOLUTIONS

At the beginning of Series 1-2 comparatively few erroneous draws were made, but the percentage of errors increased rapidly for some time with the increase in the number of beads. This increase in the percentage of errors is shown in Table V. This table includes all of the reactions of thirteen subjects upon the non-critical numbers from 4 to 11 inclusive.

TABLE V

Number of Beads	4	5	7	8	10	11
Total Draws from each Number	222	194	282	262	297	401
Erroneous Draws from each Number	14	46	79	119	92	173
Percentage of Errors	6.4	23.7	27.8	45.4	30.9	43.1

Note the rapid increase in the percentage of errors as the number of beads advances from 4 to 8 and the high but fluctuating percentage beyond 8. The unevenness of this increase is also worthy of note. From 5 beads the percentage of erroneous draws is 17.3 per cent higher than from 4; from 8 beads it is 17.6 per cent higher than from 7. But the percentage of erroneous draws from 7 is only 4.1 per cent higher than from 5 though the interval between 5 and 7 is twice as wide as that between the members of the other pairs of numbers compared. This difference is undoubtedly due to the nature of the errors which are possible in drawing from the different numbers. The only errors possible in drawing from 4 and from 5 beads result in leaving 2 and 4 beads respectively; from 7 and 8 the only possible errors result in leaving 5 and 7 beads respectively. Thus from the point of view of the number of beads left after an erroneous draw from each of the foregoing numbers, the long intervals occur where the change in the percentage of errors is greatest. The change in the percentage of errors in the early numbers of the series is therefore apparently due to the degree of ease with which the consequences of a given move may be foreseen in a purely perceptual manner.[15]

The perceptual character of these early solutions is further shown by the behavior of the subjects. Usually 1 or 2 beads were moved aside tentatively or otherwise marked off so that the possible result of further draws could be directly perceived.[16] This tentative manipulation was purely a trial and error affair at first, as was indicated by the constant shifting from one of the possible draws to another, as well as by the comments of those subjects who mentioned the matter at all. Sometimes both hands were employed in this sort of manipulation,—one representing the subject and the other the experimenter,—and so the consequences of all possible draws were figured out on the perceptual level. The movements used to mark off, or temporarily exclude some of the beads from attention, were not always of this overt character. Often they became almost imperceptible and occasionally verbal reactions served this function even in the early stages of the game. For some subjects these perceptual judgments were important factors in the solution of problems far beyond the immediate span of attention. Thus some of the subjects, upon discovering late in the game that 6 or 9 could not be won, would mark off and exclude so many beads from attention and then proceed to solve the remaining numbers upon the perceptual level. But usually when the number of beads was increased to beyond 7 or 8, the perceptual form of solution more or less completely broke down.

---

[15] The large downward fluctuation in the percentage of errors at 10 offers no serious difficulty to this view. This fluctuation occurs where it might most reasonably be expected; i.e., immediately after one of the long intervals and after the range of direct perceptual control has been passed.

[16] If the hold upon the beads thus tentatively drawn aside was released the maneuver was counted a draw; therefore the caution, and also the difficulty of applying perceptual checks upon the higher numbers.

The important feature of these perceptual solutions is the fact that all progress was here made through trial and error perceptually checked and entirely without the use of symbols except an occasional word used in a very specific way. That is to say, there was a conspicuous absence of generalization in these early perceptual solutions, due to the lack of any necessity for the use of symbols. As will be seen later, the failure to utilize symbols in these early problems seriously limited the transference of control from the lower to the higher problems of the series.

D. ANALYSIS

Immediately following upon the period of perceptual solutions there was usually a period of evident confusion. Though subjects had been instructed at the beginning of the series to look for underlying principles, they generally failed up to this point of the game to see any relation between problems and to remember how or understand why certain numbers had been won. In accordance with James's view that all analysis depends upon the "law of varying concomitants" or upon the elements having somehow previously been brought to attention in isolation, further progress here would require either that the solutions of earlier numbers be recalled and applied to the problems at hand or that some sort of manipulation be carried on by means of which the significant elements of the various problems could be abstracted and associated with appropriate reactions. Both of these alternatives were tried, the former without success in a single case when attempted early in Series 1-2, the latter with varying degrees of success, depending on the individual who made the attempt and the sort of manipulation resorted to.

The sorts of analysis which occurred are classified from the points of view of (1) the specific elements and types of elements which were abstracted and employed for generalization, (2) the explicitness and extent of analysis, and (3) the temporal relations of manipulation and ideational analysis.[17]

1. *Types of Elements Abstracted: The Direction of Analysis.*

Any aspect or isolable portion of the objective situation and any relation between such portions or aspects will hereafter be referred to as an "element" of the general problem. Elements of this sort appeared in considerable variety and exhibited uniformities of such varied types that different subjects might conceivably have arrived at equally valid solutions from quite different lines of approach.

Subjects usually began early in Series 1-2 to count the number of beads from which they were required to draw at each move. This usually led to an early discovery of the fact that certain numbers are especially significant as points of orientation and control in the series. Certain multiples of 3 were usually the first numbers to take on this special significance in Series 1-2. Thus subjects almost invariably came to regard the numbers which they could not win as the important elements to be sought out. Sometimes subjects also tried to remember the numbers which they were able to win, but these numbers were seldom made the objects of special attention and the basis of hypotheses and generalizations, except as they were brought in to complete the formulation after the solution had been practically worked out upon some other basis.

Perhaps the next most common type of element to attract special attention was the relationship of the draws made by the experimenter to those of the subject. Occasionally a subject would become so absorbed in the pursuit of this relation that he would utterly fail for a time to notice the significance of the number of beads presented. Most of our subjects, however, noticed some sort of relationship here rather early and divided their attention between it and the number element mentioned above.

Other elements of particular interest at times to various subjects were the number of beads obtained by the subject or by the experimenter or both, or sometimes the total number of beads drawn, or the number of draws obtained by the experimenter or by the subject, or the relation between the number of draws and the number of beads obtained, etc. Though elements of this sort all exhibit uniformities of such character as might well become the basis for successful generalizations, no one succeeded in getting a successful solution from these elements alone, although some subjects lost a considerable amount of time in the attempt.

---

[17] See Ruger, "The Psychology of Efficiency," pp. 10-14.

Without going further into detail it may be said that the more successful subjects usually began very early to give special attention to the number of beads remaining after each draw and often failed entirely to notice the relation between the draws of the subject and those of the experimenter. Often in the first series, and quite generally in the later series, elements of both of these types were constantly taken into account with good results. The attention of the less successful subjects usually fluctuated considerably between the different types of elements, but failed to follow up any type consistently enough to discover the uniformities lying beneath the surface.

Table VI shows the point in the first series where each subject gave the first evidence of having become definitely aware of the uniformities in the elements of each of the first two types mentioned above. This, of course, implies a considerable amount of previous attention to the elements underlying the uniformities. For example a subject would often study his own successful draws and those of the experimenter for some time before becoming aware of the "opposite"[18] relation between them; or he might realize for a considerable time that certain definite numbers are critical without noticing that they are multiples of 3.

It will be noticed that a number of the most successful subjects seemingly failed to discover the principle of drawing by

---

[18] In all trials beginning with a critical number E drew 1 when S drew 2, and 2 when S drew 1. Likewise, in order to win any non-critical number, S had to draw the "opposite" of E's preceding draw throughout the trial after first having reduced the number of beads to a multiple of 3 at his first draw.

TABLE VI

Subject	Point in Series 1-2 where the principle of drawing by "opposites" was discovered		Point in Series 1-2 where it was discovered that the C numbers are multiples of 3	
i	—	——[20]	13b[19]	50th trial
ii	—	——	15b	35 " "
iii	13b	25th trial	15b	28 " "
iv	9b	25 " "	15b	96 " "
v	—	——	16b	63rd "
vi	14b	71st trial	28b	384th "
vii	6b	21 " "	18b	310 " "
viii	18b	168 " "	17b	162nd "
ix	23b	303rd "	31b	315th "
x	12b	77th "	40b	547 " "
xi	—	——	21b	53rd "
xii	13b	70th trial	10b	30th "
xiii	12b	86 " "	15b	111 " "
xiv	11b	88 " "	40b	293rd "

[19] 13b, 15b, etc., stand for the various problems in the series and indicate the initial number of beads presented at each trial in the problem.

[20] The principle of drawing opposites was not discovered in this series by Subjects i, ii, v, and xi.

opposites, and that some of the subjects who found the greatest difficulty in solving the problems discovered this principle very early in the series. By the rank method the correlation between the number of trials required for the discovery of this principle and the number of trials required to find a satisfactory solution for the series, is —.324. There is a positive correlation of .728 between the number of trials required to discover that the critical numbers are multiples of 3 and the number of trials required to find a satisfactory solution for the series. Attention on the part of the subject to the numbers of beads from which he must draw thus leads more directly to fruitful generalizations than does attention to the relation of his own *draws* to those of the experimenter.

What factors, it may be asked, determine which of the various elements shall be abstracted from the total situation in any case? This question cannot be answered fully from the data at hand but it seems worth while to point out that the order of abstraction of elements is exactly what might be expected if the principal selective factors were the relative frequency of occurrence of various situation and response elements, the relative nearness of the various elements to a goal, or end of action, and the speed of the subject's reactions. These three factors will be discussed separately in a later section. Some data concerning them may well be given here.

a. Frequency of Repetition.—As already noted the numbers in Series 1-2 which were first to attract special attention and to become objects of active research were almost invariably the critical numbers. This emphasis upon the critical numbers was the result not of sudden, comprehensive insight, but of a gradual and often tedious growth of meaning. There were, to be sure, sudden spurts and long plateaus in the progress of some subjects, as a result of the drawing in of old concepts by association. But the regularity of progress in the abstraction of elements in the first series was not often seriously obscured by these factors. The first number to be isolated and treated as especially significant was almost invariably 6.[21] Thereafter 9, 12, and the higher critical numbers followed in the order of their magnitude except as the order was affected by the influx of old concepts in the form of generalizations. The early stages of the abstraction of the various critical numbers progressed with surprising independence. The first objective signs of the process of abstraction appeared usually in the form of short delays, exclamations, and other indications of critical reaction, which, if we may trust the comments of subjects, were accompanied by occasional fleeting insights into the nature of the situation. Continued repetition of these numbers served to give further emphasis to them and greater depth and stability to the erstwhile fleeting insights into their signifiance for the solution of other numbers, until finally it became possible to formulate their relations into satisfactory principles of control.

[21] It is true that prior to the recognition of the critical significance of 6, 3 was usually recognized in the concrete as a losing number. But, owing apparently to the easy perceptual control of 3, it was almost never mentioned explicitly as a losing number, except as an afterthought in rounding out a generalization which had been made upon the basis of higher critical numbers.

Corresponding to the gradual abstraction of certain critical numbers and the order in which these numbers were affected by the process, is the gradually increasing excess of the subjects' reactions to critical numbers over their reactions to non-critical ones. Not only does this excess of draws from critical numbers increase from problem to problem, but the ratio of draws from critical to those from non-critical numbers also increases rather steadily. The regularity of this increase, both in the excess of draws from critical numbers and in their ratio to the draws from non-critical numbers, may be seen in Table VII. At the left are listed the various problems of the series from the first problem, in which 4 beads are presented, to the twelfth problem, in which 15 beads are presented for solution. The figures in the succeeding columns indicate the total number of times ten subjects were required to draw from each of the numbers listed above the columns, in the solution of the problem opposite which the figures occur and all preceding problems. Thus in the solution of the problem, 4b, the subjects drew 12, 14, 0, and 28 times from 1, 2, 3, and 4 beads respectively. In the solution of 4b and 5b they drew 23, 26, 5, 28, and 26 times from 1, 2, 3, 4, and 5 beads respectively, and so on. Or reading *down* the column, under 3 for example, the ten subjects drew 0 times from 3 while working upon 4b; 5 times while working upon 4b and 5b; 46 times while working upon 4b, 5b, and 6b, and so on. The ratios of draws from critical numbers to the averages of those from adjoining non-critical numbers, are given in italics.

The frequency of subjects' draws from any critical number does not greatly exceed that of their draws from adjacent non-critical numbers until after the game has progressed to a point considerably beyond the critical number in question. Thus the frequency of draws from 3 beads becomes greater than from 2 or 4 beads only after considerable work upon 6 beads; and the frequency of draws from 6 beads becomes greater than from 5 or 7 beads only after some work upon 9 beads, etc. This is in entire accord with the fact that the status of these numbers (i.e., whether critical or non-critical), was in most cases forgotten shortly after the commencement of work upon higher numbers,

Table VIII

Total Number of Draws of ten Subjects from various Numbers in Solving the first twelve Problems of Series 1-2.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
4b	12	14	0	28											
5b	23	26	5 *.19*	28	26										
6b	23	26	46 *1.70*	28	26	41 *1.58*									
7b	35	41	61 *1.47*	42	43	41 *.94*	44								
8b	44	54	77 *1.45*	52	58	55 *1.08*	44	38							
9b	44	54	152 *2.87*	52	58	130 *2.55*	44	38	75 *1.97*						
10b	65	69	241 *3.28*	78	79	195 *2.32*	89	88	75 *.69*	131					
11b	108	78	408 *4.08*	122	101	333 *2.66*	149	151	172 *1.22*	131	220				
12b	108	78	533 *5.33*	122	101	458 *3.66*	149	151	297 *2.11*	131	220	125 *.56*			
13b	137	98	669 *5.35*	152	124	578 *3.66*	192	186	385 *2.03*	194	291	125 *.52*	186		
14b	163	109	758 *5.28*	178	137	657 *3.62*	226	208	447 *2.01*	236	339	161 *.61*	186	126	
15b	163	109	845 *5.89*	178	137	744 *4.09*	226	208	534 *2.41*	236	339	248 *.96*	186	126	87 *.69*

(See preceding paragraph for explanation of table.)

and was re-discovered only after a considerable number of trials during which the frequency of repetition of forgotten critical numbers became gradually more preponderant over that of forgotten non-critical numbers. Thus, after working for a while upon 7 or 8 beads, the subject usually forgot that he had been unable to win 6 and did not often re-discover the fact until after some work upon 9 beads or even upon higher numbers; that is, until the number of draws from 6 had come to be greatly in excess of those from 5 or 7 beads. The behavior of subjects toward 9 and 12 beads, and often toward higher numbers, was similar in this respect. But, owing to the increasing influence of generalization and other less conscious sorts of transfer, the preponderance of draws from the critical numbers became gradually less marked or entirely vanished.

Though in the work of these subjects the order in which the various critical numbers acquired their special significance is closely paralleled by the relative frequency of their presentation, it might be questioned whether this order was not determined by the order of their original presentation as problems of the series rather than by the frequency of subjects' reactions to them. That the order of the original presentation of these numbers as separate problems is not the determining factor, however, is evident from the facts presented in Table VIII where the critical numbers were not presented as separate problems prior to their discovery. The discovery of a critical number was usually not a sudden event, but a gradual process of isolation and of growth of meaning. The process will be referred to hereafter as the abstraction of critical numbers. Various stages of the process must be distinguished and defined before quantitative comparison becomes possible. The first stage in the abstraction of a critical number may be regarded as completed when, with this number of beads concretely before him, the subject gives the first clear indication of his realization that in the current trial defeat is inevitable. This recognition of defeat may be expressed in a shaking of the head or in some form of exclamation, such as, "I lose," "No use," "You win," "I cannot win now," or "No matter how I draw now you can win." Such recognition of defeat often comes in the form of a perceptual judgment, without explicit awareness of the exact number of beads remaining or of the impossibility of ever winning from this particular number. The second stage in the abstraction of a critical number will be regarded as completed when the subject clearly states the recognition of his inability *ever* to win the number in question. The third stage is regarded as completed when the subject announces his conviction that the number in question is also critical for the experimenter; i.e., that the experimenter must inevitably lose if required to draw from that number. In the interpretation of reactions indicative of the various stages the general context must be taken into account; our criteria are not, therefore, wholly objective.

The individual records of twelve new subjects, reacting to 14 beads as their initial problem, are given in Table VIII. The subjects are listed at the left as S1, S2, S3, etc., according to the number of trials required to win two trials in succession from 14 beads. In the *f* columns of the various A sections (i.e., 3A, 6A, 9A, and 12A) of the table are given the numbers of draws made by individual subjects from each of the critical numbers during the first stage of its abstraction. Similar data for the second and third stages of abstraction are given in the B and C sectons of the table respectively. The totals of draws from the two non-critical numbers adjacent to each critical number, during the various stages, are given in the *s* columns of the respective sections. Thus the record of S3 shows that only the first critical number was abstracted. During the first stage, 3A, of this process two draws were made from 3 beads and none from 2 or 4 beads. Prior to the completion of Stage 3B, 39 draws were made from 3 beads and a total of only 6 from 2 and 4 beads. Before 3C was completed 74 draws had been made from 3 beads though the total number of draws from 2 and 4 beads was only 16.

In only 4 of the 73 pairs of *f*- and *s*-column figures here presented does the number of draws from a critical number fall short of the sum of all draws from the *two* adjacent non-critical numbers, and in no case does it fall below the average number of draws from the two adjacent non-critical numbers. The average number of draws from critical numbers at the completion of Stage A in their abstraction, is 31.2; that of draws from adjacent non-critical numbers is 4.2. At the completion of Stage B the corresponding averages are 51.6 and 9.9 respectively; and at the completion of Stage C they are 100.6 and 21.8 respectively. The total number of draws from all critical numbers in all stages of their abstraction as here reported, is 3787. From non-critical numbers—though there are twice as many of them as of critical numbers—the total number of draws is only 703. That is to

TABLE VIII
Frequency of Subjects' Draws from the Critical Numbers at the Completion of the various Stages of their Abstraction.

	3A		3B		3C		6A		6B		6C	
	f	*s*	*f*	*s*	*f*	*s*	*f*	*s*	*f*	*s*	*f*	*s*
S1	1	0	55	2	167	31	52	4	57	4	158	40
S2	4	1					7	1				
S3	2	0	39	6	74	16						
S4	1	0	28	4			3	0	14	2		
S5	1	0					4	0	20	0	43	14
S6	1	0							11	1		
S7	3	1			11	5	2	1	12	8		
S8									22	5	68	26
S9	1	0					27	2				
S10	2	1					76	15				
S11	7	0					19	0	66	20		
S12	7	1					7	1	90	9		
Total	30	4	122	12	252	52	197	24	292	49	269	80
Av.	2.7	.4	40.7	4.0	84.0	17.3	21.9	2.7	36.5	6.3	89.7	26.7

	9A		9B		9C		12A		12B		12C	
	f	*s*	*f*	*s*	*f*	*s*	*f*	*s*	*f*	*s*	*f*	*s*
S1	57	13	64	13	148	50	104	74			108	82
S4	15	2	21	5			17	8	22	10		
S5	24	12	27	12	33	24	32	40			33	42
S6			11	7			80	28	83	31		
S7			22	26	52	34	19	35				
S8	24	27	38	27	55	39	29	37				
S10	98	41	103	41	118	48						
S11	36	11	85	70			60	51				
S12	105	11	125	11	251	45	164	149	172	149	190	160
Total	359	117	496	205	657	240	505	422	277	190	331	284
Av.	51.3	16.7	55.1	22.8	109.5	40.0	63.1	52.8	92.3	63.3	110.3	94.0

say, at the time of completion of the various stages of abstraction of the critical numbers, the number of draws from them is on the average more than ten times as great as from adjacent non-critical numbers.

The order of abstraction of the various critical numbers is also in practically complete harmony with the order of frequency of subjects' reactions to them individually. Not only are a subject's draws from a lower critical number invariably more numerous than from a higher one, but his draws from the lower non-critical numbers are also fewer than from higher ones. Both of these facts tend to give greater emphasis to the lower critical numbers and favor the abstraction of critical numbers from lower to higher in the order of their magnitude. A comparison of the actual order of abstraction requires that different stages of the process be listed separately. This has been done in Table IX.

As before the subjects are listed at the left. The critical numbers appear at the top of the columns, the various stages being segregated as indicated by the capitals. The figures in the columns indicate the order in which the critical numbers emerged from the various stages of abstraction. Thus the record of S1 shows that 3 was the first number to emerge from Stage A, 6 the second, 9 the third, etc. Points where hesitation and other inconclusive signs showed the probable occurrence of abstraction though no clear indications of the process were observed, are marked with asterisks. No overt indications of abstraction were observed for the blank points in the table. When a given stage in the abstraction of two or more critical numbers was completed simultaneously, they were given the same value in the table.

TABLE IX
Order of Abstraction of Critical Numbers.

	3A	6A	9A	12A	3B	6B	9B	12B	3C	6C	9C	12C
S1	1	2	3	4	1	2	3	*	1	1	1	1
S2	1	2	–	–	–	–	–	–	–	–	–	–
S3	1	–		–	1	–	–	–	1	–	–	–
S4	1	2	3	4	1	2	3	3	–	–	–	–
S5	1	2	3	4	–	1	2	*	–	1	1	2
S6	1	*	*	2	–	1	2	3	–	–	–	–
S7	2	1	*	3	–	1	2	*	1	–	2	–
S8	–	*	1	2	–	1	2	–	–	1	1	–
S9	1	2	–	–	–	–	–	–	–	–	–	–
S10	1	2	3	–	–	–	1	–	–	–	1	–
S11	1	2	3	4	–	1	2	–	–	–	–	–
S12	1	2	3	4	–	1	2	3	–	–	1	1

That the correspondence between the frequency of reaction to various critical numbers and the order of their abstraction is really very close, is shown by the fact that in only one case (S7, 3A) in the entire table did a lower number emerge later than a higher one from a given stage of abstraction. There is, however, a strong tendency in this group for all critical numbers to emerge simultaneously from Stage C. This is due to the fact that abstraction has here reached a sufficiently advanced stage to permit generalization to become effective.

b. Effect of Nearness to a Goal.—As already noted the frequency of reaction to the various critical numbers varies directly with their nearness to 0, the immediate goal of every trial. It is therefore impossible to measure separately the effects of frequency of repetition and those of the nearness of elements to a goal. Logically the relative nearness of elements to a goal resolves itself into two distinct factors: (1) the greater ease with which the consequences of tentative or hypothetical draws can be foreseen from lower than from higher numbers and (2) the closer temporal proximity of the goal to reactions toward lower than to those toward higher critical numbers. Both of these factors were probably operative as will later be pointed out more fully. Some evidence of the effects of (1) is found in the percentages of erroneous draws from various non-critical numbers as reported in Table VI. In the further presentation of evidence at this point no effort will be made to distinguish between the effects of these two factors.

That the critical character of 6 is more easily discovered than that of 9, is shown by the number of trials required by 24 subjects from each of these numbers in discovering the impossibility of winning it. The total numbers of trials upon 6b and 9b were 126 and 194 respectively. Seventeen of these subjects required fewer trials upon 6b than upon 9b. Likewise fewer trials were required upon 9 than upon 12 beads, both by the group as a whole and by a majority of the individual members. But, owing to the rapidly increasing effects of previous learning upon the higher numbers of the series, the difference here is not so great as in the former pair.

The comparative difficulty of abstraction of various critical numbers is further shown in Table VIII. The averages as they stand show a marked general increase in the number of reactions required to raise successively higher critical numbers out of a given stage of abstraction. But this evidence is open to the objection that the averages do not all represent the work of exactly the same group of subjects. The results are not much modified, however, when the work of those subjects whose records do not show complete data for the stages compared, is left out of account. In the work of 9 subjects whose records contain data for both 3A and 6A there is only one case, that of S7, in which 3A offers more difficulty than 6A. The average numbers of draws are 3 and 21.9 for 3A and 6A respectively. Stages

6A and 9A are both represented in the work of 6 subjects all of whom required more draws from 9 in the attainment of 9A than from 6 in the attainment of 6A. The average numbers of trials are here 26.7 and 55.8 for 6A and 9A respectively. Data for both 9A and 12A are contained in the records of 6 subjects all of whom required more draws for the attainment of 12A than of 9A. The average numbers of draws are here 43.5 and 67.7 for 9A and 12A respectively. Without going into further detail it may be said that the results in the B Stages show the same marked tendency for higher critical numbers to require many more reactions for their abstraction than are required by the lower ones. The less marked tendency in this direction in the C Stages is clearly due to the onset of effective generalization and need not concern us here.

c. Effect of Speed of Reaction.—The ease with which the principle of drawing by "opposites"[22] is discovered appears to depend largely upon the speed of reaction of the subject. The subjects who reacted rapidly almost invariably discovered the "opposite" relation between their draws and those of the experimenter early in the game, but those who reacted slowly usually became aware of this relation late or not at all. Data regarding the speed of reaction and the number of trials required by subjects of Groups I and III are presented in Table X.[23] The data for Group I are shown in the upper section of the table, those for Group II in the lower section. Since the extent of the series is not the same for different subjects, early or late discovery of the principle of drawing opposites must be determined not by the absolute number of trials required for the discovery, but by the percentage of all reactions upon Series 1-2 which sufficed for the discovery of the principle. These percentages are given in the last column of the table.

---

[22] That is, drawing 1 when the opponent draws 2, and 2 when the opponent draws 1. This procedure obviates the necessity of counting the remaining beads when their number has once been reduced to a multiple of 3.

[23] Similar data cannot be given for Group II owing to the fact that no separate records were kept of the time required for the solution of individual problems of the series.

TABLE X

Speed of Reaction as Related to Ease of Discovery of the Principle of Drawing by "Opposites."

Subjects	Av. No. of seconds per draw in discovering the principle of opposites[24]	No. of trials required for discovery of the principle of drawing by opposites	Per cent of total trials upon Series 1-2 required for the discovery of the principle of drawing by opposites
i	10.25 (5.2)[25]	—	1.00
ii	12.28 (12.8)	—	1.00
iii	5.94 (5.2)	25	.89
iv	5.74 (6.0)	25	.26
v	12.76 (6.4)	—	1.00
vi	2.86 (3.4)	71	.21
vii	5.05 (4.0)	21	.06
viii	3.67 (3.1)	167	1.00
ix	4.88 (4.5)	308	.96
x	4.56 (6.1)	77	.13
xi	6.07 (13.5)	—	1.00
xii	4.98 (5.1)	70	.38
xiii	2.59 (3.1)	86	.52
xiv	3.18 (5.7)	88	.24
1	6.88 (5.8)	32	.57
2	7.49 (5.3)	65	1.00
3	13.13 (11.6)	—	1.00
4	3.96 (7.2)	—	1.00
5	4.85 (4.4)	—	1.00
6	2.70 (3.1)	22	.31
7	6.16 (5.8)	35	.26
8	6.08 (5.6)	107	.75
9	5.13 (5.5)	—	1.00
10	3.83 (3.9)	63	.24
11	3.57 (4.1)	111	.39
12	3.78 (3.1)	84	.24

[24] Or until the completion of Series 1-2.

[25] In the parentheses beside the second column the average number of seconds per draw in the first fifty draws of each subject, is given.

Note the general correspondence between the ease of discovery, as indicated by the percentage values in the last column, and the speed of reaction as indicated in the second column of the table. The average time per draw for subjects who did not discover the principle of drawing opposites was 10.34 seconds in Group I and 6.77 seconds in Group III. The averages for subjects who did discover the principle were 4.35 and 5.06 for Group I and Group III respectively. By the rank method the correlation between the ease of discovery of the principle of opposites and the speed of reaction is .668 for Group I and .516 for Group III.

It might be supposed that these correlations are due to an increase in the speed of reaction after the discovery of the principle of opposites but before its announcement. That the correlations were perhaps raised somewhat by this tendency is not denied. But the effect of this factor was probably slight since the subjects were urged repeatedly to notify the experimenter of any new discovery or seemingly relevant new idea at the time of its occurrence. Moreover, the records show that subjects who reacted slowly at the beginning of the work usually failed to discover the principle of drawing opposites, whereas subjects who drew rapidly from the beginning were likely to discover the principle early in the game. Thus the average time per draw of the first fifty draws of Subjects i, ii, v, and xi, who did not discover the principle, is 9.47 seconds, whereas the average time of the remaining subjects of this group, all of whom discovered the principle, is only 4.42 seconds. The corresponding averages for subjects of Group III who did and for those who did not discover the principle, are 7.20 and 4.60 seconds respectively. The correlation between ease of discovery of the principle of opposites and the speed of reaction in the first fifty draws is found to be fairly high. For Group I this correlation is .373 and for Group III it is .629.

It may still be argued that the speedy reaction of some subjects was merely the result of their attention to the relation between their own draws and those of the experimenter; but such comments of subjects as we have, together with their time records, show that even while attending to the number of beads remaining or recalling their success or failure with certain numbers in past trials, these subjects reacted with less deliberation than others. As shown by their comments, the work of these subjects was characterized by incessant fluctuations of attention between relatively isolated elements or by wholly irrelevant reflections. These subjects appeared to react quickly not so much because of any marked speed of mental activity as because of a lack of those deeper insights into relations and consequences, which might have barred hasty reaction.

In striking contrast with these positive correlations between speed of reaction and relative ease of discovery of the principle of drawing opposites, are the correlations between speed of reaction and facility in finding a solution for the entire series, as measured by the number of trials required for the solution of the series. The average speed of reaction in the entire series for all subjects of Groups I and II, together with the number of trials required for the solution of the series, is shown in Table XI.[26]

TABLE XI

Speed of Reaction as Related to the Number of Trials Required for Solution of Series 1-2.

Subjects	Average No. of seconds per draw in Series 1-2	No. of trials required for the solution of Series 1-2
i	10.25	50
ii	12.28	35
iii	5.52	28
iv	6.89	96
v	12.76	62
vi	3.19	331
vii	4.00	371
viii	3.67	167
ix	4.60	315
x	4.33	597
xi	6.07	53
xii	6.54	184
xiii	3.68	166
xiv	2.77	367
A	12.22	48
B	7.00	62
C	5.61	85
D	11.86	112
E	6.70	220
F	11.67	300
G	4.68	322
H	7.00	389
I	4.86	617
J	3.91	823

The data for Group I are given in the upper portion of the table and those for Group II in the lower portion. The correlations between speed of reaction and the number of trials required for the mastery of the series are —.748 for Group I and —.686 for Group II, when the speed of reaction is stated in terms of the average time per draw in the entire series. When the speed of reaction is determined on the basis of the first fifty draws of each subject, the correlation is —.264 for Group I. Thus intelligence as determined by the ease of solution of Series 1-2, does not affect the speed of reaction so much at the beginning of the series as later when there is a greater accumulation of experience which may serve to inhibit hasty reaction. Speed of reaction appears then in the double rôle of an effect of the depth of insight into the conditions of the problem and as a factor in determining the direction of analysis.

---

[26] Similar data are not available for Group III owing to the failure of most of the subjects of the group to obtain a solution for the series while solving the limited number of problems presented to them.

2. *Explicitness and Extent of Analysis*

In the analysis of puzzles Ruger reports a "wide variation of felt clearness from extremely vague to perfectly clear. This range of felt clearness," he finds, "is matched by differences in results." Our results show a similar variation in the explicitness of analysis from cases in which an element of a situation is barely recognized in passing, to those in which clear verbal formulations are accompanied by ability to recognize and control the element in question under novel conditions. Numerous cases were observed where analysis was complete enough to be effective for manipulation but not clear enough to be put into words. A great many of the perceptual solutions were of this character. The analysis of the lower numbers practically never reached the explicitness of a clear verbal formulation until generalizations made upon higher numbers were applied to them. Occasionally a subject would declare that he knew how to win a given number but could not express it in words. One subject, after winning from 20 beads twice in succession declared that she believed she could do it again though she could not say how. She then proceeded to win two more trials in succession without difficulty. Another subject, having just won twice in succession from 10 beads, was asked whether she had any new ideas. She replied: "I can't state the idea in words but I have it in motor terms."

Ruger mentions several different sorts of analysis which affected only a part of the situation. A very indefinite type of partial analysis he describes as "picking out the portion of the puzzle to be attacked. In many cases," he says, "there was a mere spatial analysis, 'locus analysis' without involving any perception of the mechanical necessities concerned." This locus analysis resulted in the confining of random movements to a portion of the puzzle. This is very similar to a common experience of our subjects many of whom quickly discovered that the crucial point of the trial lay in the early draws, and busied themselves, accordingly, with the task of determining what the first one or two draws must be. This often resulted in some impatience with the requirement that every trial must continue until the number of beads was reduced to 0, and necessitated a few brief departures from the rule. Often the point conceived of as crucial was in the lower and sometimes in the intermediate numbers rather than in the higher ones, as the following examples show:

"I wonder if you often draw 2 at the beginning as I do, merely to hurry and reduce the number of beads" (xiv, 23b).[27]

"No matter what number you are working with you can win if you can get it to 4 or 5 beads. The problem isn't with the larger numbers but with the few at the end" (xii, 11b).

"It seems to revolve about the start" (xii, 16b).

"Well, I would eliminate the beads until I got my first turn on 4 or 5 (xii reacting to the problem of 59 beads).

"When I win, the crucial draw is the second, or not later than the second" (xiv, 25b).

A slightly more definite type of analysis is described by Ruger as follows: "An important form of partial analysis noticed was that of a single step in the process while the other steps were attained only by random movement. This single step was often the final one. The solution would come accidentally but the subject would notice the last step. In subsequent trials he would know what to do if he chanced to get to that step but not how to get there." The folowing comments from subjects indicate this type of partial analysis:

---

[27] The Roman numerals in parentheses indicate the subject whose comment is quoted and the numbers following show the particular problem in the series, during the solution of which the observation occurred.

"I can't beat when we get to 6. I never have beaten you on 6" (xiii, 12b).

"I can't win this; there are 6 left" (ix, 11b).

"I could win if it would come out 11 there" (ix, 14b).

"I know I can't win unless I can make it 13 there . . . after you have drawn" (vi, 25b).

"Some of the first moves I win and others I lose. Now on 5 if I have the first move, I win" (vii, 22b).

"I was trying to get it so there would be 7 beads left on my draw" (viii, 12b).

These partial analyses, especially when concerned with the higher numbers in a series, depended largely upon the possession of some general scheme by means of which attention could be freed from most of the details of the series and concentrated upon some one point. Such schemes usually took the form of drawing uniformly either 1 or 2 beads throughout the trial, and generally resulted quickly in some sort of appreciation of the principle of drawing by opposites. This principle is one of the best examples found here of what Ruger called "schematic analysis." A partial insight into the significance of multiples of 3 sometimes served as a schema of this sort. One subject, for example, expressed a schematic view of the line of approach to a solution of Series 1-2 as follows: "You draw 2 when I draw 1, and 1 when I draw 2, so that we reduce it by 3's each time; that is, we are drawing them out by multiples of 3" (vii, 18b). From this comment it might be thought that the subject had effected a complete analysis of the series, at least up to and including 18 beads; but it required two additional trials to bring him to realize that he could not win 18 beads, and many more trials to complete the series. During the next trial he observed: "We are drawing right down on multiples of 3"; but here again he failed to realize the significance of multiples of 3, for it required still another trial to convince him that he could not win 18 beads, and his conclusion was not based on multiples of 3 at all. "I can't win from 6," he said, "so since this is a multiple of 6, I can't win it." Of the problem presented by 18 beads the subject had probably made a total analysis in the sense that it "reached all the elementary steps or movements." That it was far from a complete analysis of the entire series is evident from the fact that it required still 45 trials upon 19, 20, 21, and 22 beads for the development of sufficient insight to permit the subject to give a solution for the entire series. At this stage of the process, however, analysis proceeded far less than formerly by frequency of repetition, nearness to a goal, etc., and far more by means of ideas and principles formulated in early problems and now applied to new problems and verified with a minimum of repetition. In other words, generalization, which has played a rôle of increasing importance since the earliest stages of analysis, has here become the dominant factor.

3. *Time Relations of Manipulation and Analysis*

In describing the time relations of ideational analysis and motor variations Ruger says: "These two types of variation, acts of analysis and motor responses, may be quite varied, especially in their time relations. At the one extreme is the motor variation which, perhaps, brings success but which runs its course unnoticed. At the other extreme the analysis may occur first and only after a considerable interval be followed by the motor response."[28] This wide variation in the time relations of analysis and manipulation is very characteristic of the work of our subjects, except that in the first series we find little ideational analysis preceding manipulation. Our procedure was not of course such as to encourage attempts at analysis prior to manipulation in the early problems of the series, though we believe that such efforts were seldom discouraged except by their failure to bring results. Such analysis as occurred early in the series was usually of a perceptual nature, as already explained, and was accompanied and checked by manipulation of the beads. The extent to which the method of trial and error became operative in the intermediate and higher numbers of the series, is indicated roughly by the percentage of erroneous draws. These percentages for some of the subjects in the first six problems of the series are given in table V. In approximately the first two-thirds of the trials of a subject upon each of the higher numbers the erroneous first draws usually amounted to about 50 per cent of all of the draws from the number in question. Often, indeed, half of the first draws of all trials in a number of successive problems, were erroneous. Thus Subject viii, in working from 7 to 14 beads in the first series, made 86 errors of this sort out of a possible 175. In the 7 problems from 8 to 14 beads inclusive the possible errors for this subject were 156 and his actual errors were 86. Subject i required 38 trials in working over the numbers from 7 to 14 inclusive in the first series; i.e., there were 38 possibilities of erroneous first draws. Although this subject was one of the most successful of the lot and made special effort at analysis before manipulation, he made 21 errors in the 38 draws. His comments upon his methods of procedure are of interest here: "I'm doing this by the trial and error method until I get it down to 4 or 5 where I can handle it." Later he says: "I find that any attempt to analyze this series is apt to inhibit action; at least it doesn't conduce to a solution." Repeated comments of this nature were made by many of the subjects.

---

[28] *Op. cit.*, p. 12.

It often happened, while drawing through a trial which had been given up as lost, that success would be accidentally achieved. Frequently the subject would attempt to recall how it was done. Often he succeeded, but even if he failed, the effort was not entirely lost, for the arousal of attention to the existence of neglected possibilities resulted in a change of attitude and sometimes in a fruitful examination of his assumptions. Numerous occurrences of accidental success without ability to recall the successful variation were noted. The following case is representative:

Subject ix had experienced particular difficulty with 11 beads, Series 1-2, and finally concluded that he could not win if the experimenter drew 1 early in the trial. He was persuaded to continue, however, and finally happened to take 2 at every draw and so won the trial. His comment was: "I just fell on to that by trial and error procedure. It was accident; I don't believe I could do it again." Some time afterwards he met with the same difficulty when drawing from 14 beads. After winning with considerable difficulty, he said: "I don't know how I did that." Later on with 17 beads he repeated his old error seemingly none the wiser for his two accidental successes.

A striking example of how accidental successes may be seized upon and made the basis for further progress is found in the reactions of Subject vi who, having made good progress up to 25 beads in Series 1-2, became stranded and required 156 trials to win this number twice in succession. This subject soon discovered that when his initial draw was 1 and the experimenter drew 2, he could win by taking 1 at every draw throughout the trial. But when the experimenter took 1 at the first draw, the subject was helpless, though he could have won by merely changing his draws to 2 from this point to the end of the trial. Somehow the latter procedure escaped his attention, although he followed it once by accident near the beginning of the plateau, so that with 57 opportunities to win by this procedure he succeeded only twice. After his 155th trial upon 25 beads, when the successful variation occurred for the second time, he said: "That's another way I win—certainly I could win that way if I won the other way!" When questioned as to whether he had known from the beginning of the trial that he would win, he replied: "Not until the third to the last draw. In fact, I started drawing 2 without any intention of continuing to draw 2 on down through the trial. Then I later decided to continue (because, as subsequently brought out in a comment, he did not remember having tried that before) as I started. . . . It didn't occur to me until after I had finished, that it didn't make any difference whether I got that one extra bead first or last, if you drew 1 each time."

The last sentence throws some light on the nature of the difficulty experienced by him with this combination. If the subject and the experimenter drew 1 bead each at the first draw, there would be 23 beads remaining. Now, if the subject took 1 bead at his second draw and the draws proceeded by opposites after that, there would be 2 beads left for him at the last draw; that is, "1 *extra* bead." The subject's reactions clearly show that some such assumption as, that he must provide for that "extra bead" at his first opportunity, was made; for of the 55 errors which he made in drawing 1 when he should have taken 2, forty consisted in drawing 1 from 23 beads. The other 15 errors of this sort can be accounted for largely on the basis of his efforts to reach certain lower numbers which he was gradually learning to regard as significant. This false assumption explains why the subject made 40 errors in 57 draws from 23 beads. The case shows how an accidental success may serve to clear away false assumptions and so contribute indirectly as well as directly to a more speedy solution of the problem.

It is worthy of note that the accidental successes in the foregoing cases, which occurred before a fair acquaintance with the series had been formed, either failed to attract attention or could not be recalled, whereas an accidental success occurring later in the series was quickly seized upon and applied to the solution of the series. This was quite a common occurrence in the work of a majority of our subjects.

Having seen the futility of early attempts at analysis without manipulation, and the varying degrees in which subjects profited by accidental success at different stages of their mastery of a series, we turn briefly to the growing capacity for ideational analysis, which usually began to be effective towards the end of Series 1-2 and became increasingly apparent as the experiment progressed. The first successful attempts at ideational analysis of more than perhaps one step in the series usually consisted in a return to lower numbers which were regarded as related to the present situation, with an attempt to determine their forgotten status, or in a return to the very beginning of the series to recover orientation. Reasoning about these lower numbers would invariably take the form of ideational manipulation mentioned earlier in the discussion. Usually some numbers remembered as critical served to facilitate the determination of the status of other numbers. An example will show the character of this sort of analysis more clearly:

After gaining a fair acquaintance with Series 1-2 in 371 trials upon the first 17 problems of the series, Subject vii took an excursion back to the beginning to check up and get his bearings.

"Some numbers," he said, "the first (draw) wins and some it loses. Now at 5 beads if I have the first move, I win; at 7 I lose—I take 2 and you take 2 and I lose. From 7 if I take 2. that leaves you 5 and you can take 2 and win. If I take 1 and leave 6—if I have 7 and take 1, I win. If I have 9, I lose. If I have 9 and take 1, I lose the game; if I have 9 and take 2, I lose, therefore I lose on 9 always. If I have 8 and take 1, the other fellow wins; if I take 2,—O yes! *I see why it is that I win 8, because I leave 6.* I can win 5 by taking 2 and I can win 7 by taking 1; 6 I can never win. If I have 8 and take 2, I always win. I can win 5 if I take 2; 6 I can never win, 7 I can win if I take 1, and 8 I can win if I take 2; 9 I can never win—6 and 9 I can never win; 10 if I take 2, I lose; if I take 1, I can always win. 11 if I take 1, I lose; if I take 2, I can always win. 12, if I take 2, you can take 1 and leave 9; if I take 1,—I can't win it. 5 I win and 6 I lose—on even numbers I take away 1. On 11 if I take 2—12 I lose because you can leave either 9 or 8. 6, 9, 12 I can't win. I can't win any multiple of 3. On other multiples of 5—I can't carry it out. Let's go on."

Here the subject was asked how he would draw from 59 beads. "An odd number," he said. "This will be merely a guess. I should judge that since it is an odd number, I could win. I would try to leave it odd all the way. I would take 2 and the other fellow couldn't win if I watched my draws. I think he couldn't win if I watched my draws. I think he couldn't win multiples of 3—*I would reduce him to multiples of 3!*"

Even after the solution of an entire series most of the subjects showed but slight tendency towards or capacity for anticipatory analysis in the attack upon new series. Twelve of the fourteen subjects in Group I began the attack upon Series 1-3 in exactly the same manner as that upon the first series; that is, they began to draw without delay and only after some time attempted to formulate a statement for the series. The effect of their work upon the preceding series was very noticeable, however, in the readiness with which some subjects took note of the fact that 4 could not be won, and in their alertness for other critical numbers.

Subject i said at the beginning of Series 1-3: "I am going to try to reduce this trial and error to a minimum. With 1 or 2 (i.e., in Series 1-2) the smallest combination was 3; 1, 2, or 3 is a more difficult proposition. I imagine I could sit down and work it out." He drew 2 and the experimenter took the remaining 3. "Oh, I forgot," he said "that there were three possibilities. I will try all the possibilities now and see if you can get me." From this point manipulation and analysis progressed together. This failure of one of the most successful subjects, certainly the one who was most inclined towards anticipatory analysis, shows the difficulty of such analysis in the early stages of acquaintance with the elements of the problem. His progress had been, however, very rapid in the first series so that time had not permitted a very thorough stamping in of the elements, and he had altogether failed to notice some of the important elements of the series.

When presented with Series 1-3, Subject xiv began to draw after slight hesitation but said immediately after the first trial: "Now I think I can get this right off. This time you are going to let me win every fourth one; that is, on the fourth you *won't* let me win and on the fifth you *will* let me win, provided I start with 1. On 6 I should have to start with 2, and with 3 on 7. (And on the 8th?) I wouldn't win 8." This subject quickly solved two more series of this order without a draw.

This exceptional case may well be due to the fact that Subject xiv made a more thorough analysis of Series 1-2 than was effected by any other subject. Also by a great deal of shifting of attention from one set of elements to another during 367 trials occupying 8173 seconds, as compared with an average of 204 trials in 4943 seconds for all subjects of Group I, he had probably succeeded in stamping in these elements more thoroughly than had been possible for most of the subjects. With less than half as many trials as xiv required for Series 1-2, Subject i solved in the neighborhood of thirty series of problems and gave satisfactory generalizations for all problems included in the experiment. It is clear, therefore, that the insight shown here by Subject xiv cannot be regarded as a case of ideational analysis without a fair acquaintance with the elements of the situation.

It required the solution of only one or two additional series of problems to give this control of all series of the first order (i.e., all series in which L is equal to 1) to nearly all of the subjects, and all succeeded in getting a general solution after only a few more series. But no subject solved Series 2-3 without manipulation of the beads. The solution of fewer series was required, however, for the mastery of the second order of series than for the first order. Likewise the mastery of all series of the third order required actual work on fewer series than that of either of the preceding orders, and usually resulted in the mastery of all series in which the numbers between H and L might be drawn. This transfer of power from series to series was due largely to the easy recognition of certain elements of the new series, which were identical with familiar elements of old ones, thus obviating in part the necessity for new analytic activity. The transfer of power was further facilitated by generalizations upon familiar elements, which were easily applicable to new series. These factors of familiarization and generalization became more prominent as the work progressed, but in series where some new elements were introduced there was still a ncessity for genuine analytic learning. Much of this analysis continued to follow or accompany manipulation, but a gradually increasing proportion was performed in ideational terms, largely by the sort of ideational trial and error already described. Thus with increasing acquaintance with the materials we find a continual receding of activity from overt trial-and-error manipulation to a very similar sort of process carried on in ideational terms, which in turn gives way to general ideas that have evolved gradually by the bringing together of similar elements in association with general symbols. In the later series all three of these factors were almost constantly in operation.

4. SUMMARY

After the range of perceptual solutions was passed various elements of the problematic situation were gradually abstracted and associated with verbal symbols. The type or combination of types of elements so abstracted differed largely for different subjects and in different periods of the work of the same subject. The type of elements abstracted was shown to vary in accordance with the speed of reaction. Frequency of reaction to various elements and their nearness to a goal, were shown to be closely correlated with the order of their abstraction. Large differences in the degree of explicitness of analysis were observed, and there was found to be a somewhat gradual development from vague to clear and explicit states of analysis. The extent of analysis varied with different subjects and at different stages of the learning process, but analysis usually occurred first in isolated spots and spread from these to other portions of the series. The time relations of ideational analysis and manipulation varied greatly. In general, manipulation preceded analysis in the first few series, and persisted throughout the greater portion of the learning process as an important method of procedure. Gradually, however, as acquaintance with the elements of the situation grew, overt trial and error was replaced by a very similar sort of ideational manipulation which, in turn, tended to give way to general ideas.

E. GENERALIZATION

In the preceding section attention was directed principally to the abstraction of the elements of the problems presented for solution. As was shown in several instances, however, such elements as are abstracted do not remain in their early state of relative isolation during the entire course of analysis, but tend to combine into higher units which become associated with appropriate symbols and take on general meanings. This process of generalization and the resulting general ideas will be the subject of discussion in the present section.

1. *Relative Absence of Generalization in the Perceptual Stage*

Mention has already been made of the apparent lack of generalization during the period of perceptual solutions. Practically no comments were made during this period, which would seem to indicate any attempts at generalization. Moreover, when a subject did finally attempt to generalize, those numbers which are well within the range of perceptual control were rarely taken into account, though they had been more often repeated than any of the higher numbers. Only two of the fourteen subjects of Group I noticed, with sufficient clearness to mention the fact, that 3 is a critical number, until attention was directed to it by their final generalizations which were based upon higher numbers. Some of the subjects were very much surprised that this fact had so long escaped their attention. The first important landmark mentioned by eleven of the subjects of this group was 6, and this was generally mentioned not earlier than the last trial on 9 beads, sometimes not until much later.

But this failure to recognize the status of lower numbers in the abstract does not preclude recognition of the critical or non-critical character of the concrete situations for which they stand. In fact, the situation which is represented by the critial number 3, was almost invariably the first to be reognized in the concrete as critical. This fact is shown in Table VIII where the realization of the impossibility of winning 3 is shown to have been the first step taken by ten of the twelve subjects of Group III in the abstraction of critical numbers. Table IX, however, shows that 3 was explicitly mentioned as a critical number before 6 was so mentioned, by only 2 of the twelve subjects. The failure of subjects to utilize numbers below 6 along with higher numbers as a basis for generalization, is not, therefore, due to the difficulty of recognizing their status in the concrete but rather to the ease of such recognition, which made it unnecessary to associate these situations with verbal symbols.

2. *Development of the Concept of the Critical Number*

Though the concept of the critical number seems to be a very simple affair, its development required the expenditure of a considerable amount of time and effort on the part of the subject. To facilitate the description of this development we may divide it into the following seven stages:

Stage A.—The first appearance in explicit form of the critical-number idea was at the point where the subject discovered that he could not win 6 beads. But this was straightway forgotten in most cases, or at any rate failed to function in slightly new situations. Similarly, the impossibility of winning 9, 12, etc. was later discovered without a full realization of the significance of these numbers at the time. This first discovery of the impossibility of winning a number constitutes the first stage in the growth of the concept of the critical number. Critical numbers in this stage stand out in comparative isolation.

Stage B.—Since the subject was required to draw first in every trial, it was impossible to compel him to draw from a given critical number in any trial in which the inital number of beads was only 1 greater than the crtical number in question. Therefore during the solution of the next higher number in the series the recently discovered critical number was usually forgotten. Its re-discovery after more or less delay constitutes the second stage in the development of the concept. Here the individual critical number is recognized as an element of all problems of the series; e.g., 6 is recognized as critical regardless of what may have been the initial number of beads presented in the trial.

Stage C.—At this stage the relationship between two or more critical numbers in Stage B, or perhaps in Stages A and B, was discovered; as, "I cannot win 9 because you can always reduce me to 6"; or, "I cannot win 12 because I lose 6, and 12 is composed of two 6's," etc. Here the critical numbers are not only recognized as critical in all problems of the series, but they are associated with each other in more or less definite relations.

Stage D.—The fourth stage consists in the discovery of the fact that the subject cannot win any multiple of 3. All critical numbers are recognized as critical and all are associated with a common symbol.

Stage E.—The foregoing stages all deal with the critical numbers as related to the subject's recognition of the possibility of success. A somewhat similar development may be observed in his utilization of the critical-number idea as a means of control in the achievement of success. Thus it often happened that a subject discovered the possibility of winning by reducing the number of beads to 6 and so forcing the experimenter to draw from it, long before learning that the same procedure applied to the higher critical numbers would bring the same results. In this stage one particular number is recognized as critical *for the one who draws first.*

Stage F.—In Stage F two or more critical numbers are recognized as having this broader significance.

Stage G.—Here all critical numbers are recognized as critical for the one who draws first.

It must not be supposed that learning progressed with perfect regularity from stage to stage in exactly the foregoing order, or that all of the stages were distinguishable in the progress of every subject. They do, however, occur separately in the majority of cases, and in some instances in the exact order named. The following comments of subjects will serve to illustrate this development. They are all taken from Series 1-2. The stage of development of the concept, as inferred from each comment, is indicated in parentheses. The portion of the series in which the comment occurred is in each case indicated at the left. 12b, 12, for example, signifies that the first comment occurred during the twelfth trial on 12 beads.

Subject vii

12b, 12: "You always manage to leave me 6." (B)
" 26: "I can't win 12 because you can always reduce it to 6; 12 is two 6's." (C)
15b, 11: "You reduce it to 6 every time." (B)
" 22: "You always manage to get to 6; I don't know how I am going to prevent you." (B)
" 24: "There are 12 left; I can't do much with that." (B)
" 24: "There are 9 left; I haven't been able to win any 9's." (B)
18b, 8: "Evidently this is one that you don't expect me to take because you throw it into the 6's." (C)
" 9: "Your idea is to balance my moves so as to make it 6; and 9 is just as good for you." (B)
" 15: "You draw 2 when I draw 1 and 1 when I draw 2, so that we reduce it by 3's; i.e., we are drawing them out by multiples of 3." (C)
" 16: "We are drawing right down the multiples of 3." (C)
" 17: "This is a multiple of 6; I can't win from 6 so I can't win it." (C)
20b, 1: "The subject draws two and is asked why. "I have no reason," he replies. E then draws 2 and the subject takes 1, explaining: "I draw that way to keep it on multiples of 5 rather than on multiples of 3, and so to avoid 12, 9, and 6." (C)

" 2: After drawing 2 S says: "I can't win with 18." (B) (E?)

22b, 9: "If I have 8 beads and take 1, the other fellow wins. If I take 2—Oh yes, I see why I win: because I leave 6. (E) . . . I can't win any multiple of 3. . . . I can't carry it out; let us go on." (D) The subject is here asked how he would draw from 59 beads. After some irrelevant speculations he concludes: "I would take 2 and the other fellow couldn't win if I watched my draws. I think I would reduce him to multiples of 3." (G)

Subject xiii

10b, 6: "It depends on which draws first." (E?)

12b, 30: "I can't win when we get to 6. I never have beaten on 6." (B)

" 36: "I couldn't win 6; I don't believe I can win 12. I have been trying to see how to prevent you from leaving me 6. Double it and it is the same. It would be the same on all multiples of 6." (C)

15b, 6: "I can't beat you on this; it goes by 3's. *I have forgotten whether I beat you on 3.* 6 is the first I fell down on, isn't it? I don't know about 9." (D)

20b, 2: "I can't beat you on 16—Oh no, it was 18 I lost. (B) If it is 20 and I draw first, I win; but if you draw first, I lose. Let me see, I didn't beat you on 18. If you draw first on 18, I win." (F)

24b, 2: "I don't believe I can beat 18, 12, or 9, or 6. It must be multiples of 6 I can't win." (C) (D?)

26b, 2: "When I get 26, I see that by drawing 2 first I can give you 24, and I can therefore win." (F)

27b, 2: "I don't believe I can win. You make me draw on 24 no matter how I move. It must be multiples of 3, i.e., 3, 6, 9, 12, 15, 18, etc. *That's what it is!* (D) It depends on who draws first. There are certain numbers that the one who draws first can win and others that his opponent can win. (G) . . . After your first draw, i.e., after you get it to that number, draw opposites." The subject is here asked how he would draw from 59 beads and replies: "I would take 1 and, provided you take 2 every time—let me see, there may be another element there. I must draw 1 first on the odd numbers which I can win and 2 on the even numbers that I can win. But I am not certain yet."

28b, 3: "It looks like it is even numbers that I draw 1 on; and on odd numbers I draw 2 perhaps."

29b, 1: "That's the way it is. 3's I can't win, and multiples of 3. (D) 4 and 5 I win; 7 and 8 I win; 10 and 12 I win, etc. So on even numbers I draw 1 and on odd numbers I draw 2, and the opposite after the first draw."

31b, 1: "My scheme didn't work!"

" 2: "I have missed something there."

" 3: "It is a question of making you draw first on the multiples of 3. I didn't draw 2 here so that was wrong (i.e., his idea that he must draw 2 from odd numbers, etc.). It is a question of what to draw so as to make you draw first on a multiple of 3." (G)

Subject xii

9b, 0: "The first I couldn't win was 6, wasn't it? This is 9."

" 8: "Is it possible that with 6 or multiples of 6, such as 9 and 12, one can't get any results? that is, on all multiples of 3? (D) . . . In all cases of multiples of 3 I am going to assume that I can't get any results."

12b, 9: "I am positive I can't get this. I always begin with 6 (B). . . . *I came back to the original hypothesis* and thought of 6 as a multiple of 3." (D) S is here asked how he would draw from 59 beads. His reply shows that he has failed utterly to realize the significance of his idea of multiples of 3 for the achievement of success: "Well, I would eliminate the beads until I got my first draw on 4 or 5."

14b, 6: "When there are 6 and it is my turn to draw, you can always win." (B) The subject does not realize yet that 9 and 12 are also critical numbers.

14b, 9: "You can always make up the deficiency so as to take away an even number and leave 6." (B)

15b, 1: "I am going to try out that hypothesis again, that any number which is a multiple of 3 precludes any possibility of success for me." (D)

15b, 7: "Whenever a number is a multiple of 3, it is useless for me to try it." (D)

16b, 4: "When it gets to 9, I lose. (B). In this case I am working on the first 7 out of 16."

" 16: "It seems to revolve about the start."

" 37: "No matter how I move you can always bring me to 9. I lose 12 too." (B)

18b, 0: "I'll bet a cow I can't win 18." S is here asked how he would draw from 59 beads, and replies: "I couldn't get it."

21b, 1: "I lose multiples of 3." (D). When asked how he would draw from 61 beads, he says: "I would draw 2. (Why 2?) To get to 59 and bridge over 60. (E?)

22b, 1: "Well, I lose and I thought I had it cinched." The subject later stated that he took 2 beads at the first draw in this trial in order to "bridge over" 21.

22b, 2: "I see my mistake. I took 2 and you 2 leaving 18, another multiple of 3." (D)

24b, 0: "I'll pass it up." (D)

25b, 1: "That was foolish. I took 2 and allowed you to reduce it 21, a multiple of 3, and I lose." (D)

27b, 0: "I'll pass it up." (D)

30b, 0: "I'll pass it up." (D). The subject was asked how he would draw from 82 beads. He replied: "I would take 1. No, I would take 2, because that would throw me off the multiple of 3 again." (E?) From 67 beads he said he would take 1 "to avoid 66, a multiple of 3." (D). This subject did not advance beyond Stage D in Series 1-2, although he seemed at the point of making the larger generalizations at several times.

Subject i

9b, 5: "I cannot beat you at 9. No matter how I draw you can take such a number as to reduce me to 6 which I can't win." (B)

11b, 0: "Now if I knew what my winning numbers were for 7, 8, 9, and 4, and 5, I think I could win."

" 4: "Did I win 6 before? I don't think so." (B)

" 6: "You reduce it to 6 no matter how I draw, and 6 is impossible." (B)

" 7: "The first party can't solve 11 if his opponent reduces the number to 6 (E). I have it. . . . If I can prevent you from reducing the number to 6, I can win."

12b, 3: "It reduces itself to this: Can I prevent you from reducing it to 6? You can leave 6 irrespective of what my move is. Twelve is composed of two 6's." (C)

13b, 1: "Now I want to prevent you from getting 6 or 12, so I must necessarily start with 2 (E?). (S draws 2 and E 1) Nine beads. There you have got me because I take 1 and you 2 and leave me 6, or I take 2 and you 1 and leave me 6." (C)

" 2: "I'll take the other chance and start with 1. No there is no use because I would be leaving you 12. Well, as far as I can see my only chance is to start with 2, but even then you can reduce me to 6 if I am not mistaken.

" 3: "It seems to me that I lose. If I take 1, you have 12 and win; if I take 2, you can bring it down to 9 and hence to 6 (C). If you can reduce me to 6 or 12 no matter how I draw, why I lose, that is certain. Let's see if I can beat you on 6. . . . No I can't beat you on 6, that is evident. I have an idea that it is multiples of 3 you win." (D) When asked how he would draw from 59 beads, he replies: "56 is the nearest multiple of 3 to 59. If you could reduce it to 57, you would have me. There again I can't move so as to prevent you. If I took 1, you would take 1 and leave me 57. If I took 2—Oh yes! If I moved 2 and followed it out consistently, I think I would have you beaten." (F, G?) The subject here stated that he had a glimpse of the right procedure at 11 beads, but that some distraction or other caused him to lose sight of it for a time.

Subject viii

11b, 20: "When there are 4 and it is E's draw, I can get it. My problem is to get it reduced to 4 when it is his draw. (E?)

12b, 18: "The idea strikes me that this cannot be gotten, and also that it is a multiple of 6 that cannot be beaten." (C)

13b, 18: "First I wanted to get 4 beads with my draw; I later found that I would have to work not from 4 but from 5. . . . Later I changed it to 7. I thought I could work down to 4 from 7, and then I began to work from 9. I then found that I would have to begin to check off from the total; that is, if there are 13 beads, I draw 1 and then count 12. . . . *My object was to get 9 beads when it was my draw.*" (E?)

" 33: "I counted back to 11 and worked it out from 11. Here *I tried to get my opponent down to 6 instead of myself.* (E)

14b, 12: "I forgot that I was trying to keep from getting 6, and consequently for a good while I was trying to get 6, and not being able to win caused me a good deal of confusion. I have had this mixed up for some time."

15b, 7: "I am now working on 9 instead of 6." (F)

16b, 4: "I think that the point of the game is that the one who wins has to have the other one draw on some multiple of 3." (G)

The foregoing comments of subjects are merely milestones in their progress through Series 1-2, yet if the reader will refer to the position in the series where each comment belongs, he will appreciate the extreme slowness with which the simple meanings often developed and the lack of uniformity in their attachment to various elements of the series. It has already been shown that the lower critical numbers are usually wholly neglected for a time after their first discovery and that they are later re-discovered in about the same order as that of their first discovery. In general, each new acquisition of meaning—each advance to a higher stage in the foregoing classification—began with those critical numbers above the range of purely perceptual solutions, which had been most often repeated, and spread to the higher critical numbers much in the order of their frequency of occurrence in the work of the subject. Thus 6 was the first critical number, usually, to be re-discovered, and also the first to be definitely associated with a higher critical number. So also 6 and the immediately following critical numbers were usually the elements between which the multiple-of-three relationship was first apprehended though higher critical numbers were at the time known to the subject. Likewise 6 was usually the first critical number to advance to Stage E, and the higher critical numbers followed roughly in the order of their magnitude. It has already been shown that the reactions of any subject to a lower critical number were invariably more frequent than his reactions to a higher one. Thus in the elevation of critical numbers to higher levels of meaning frequency of repetition and nearness to a goal appear to be important factors, just as in the preceding section they were shown to be factors in the direction of attention to the critical numbers.

In the elevation of critical numbers to higher levels of meaning there was, however, considerable deviation from the order of their frequency of repetition. Thus 12 was often taken up into Stage C upon its first discovery long before 9 was rediscovered. This was clearly due to the simple multiple-relation existing between 12 and 6, by which the associations already built up about the latter were made to attach immediately to the former.[29] This spread of meaning through the agency of previously acquired associations might well be expected since the elements of our problems were not only familiar to the subjects, but were also closely related in past experience. But this calling up of familiar concepts by association is itself perhaps dependent in large measure on the frequency of their previous repetition in association with some element of the present situation. These irregularities in the process of learning did not greatly alter the general character of the learner's progress in the early stages of learning. The generalization of the formula for Series 1-2 into a suitable formula for all series of the first order[30] required from 1 to 6 further series for its completion; and the generalization of the latter formula so as to make it cover all continuous series[31] required from 2 to 10 additional series. It is therefore evident, notwithstanding frequent rapid advances by means of familiar concepts which were brought in by means of association, that the development of meaning was a rather slow and fluctuating affair, presenting some striking parallels to the formation of sensori-motor co-ordinations.

Another fact of interest, which is exemplified in some of the comments listed above, is the fleeting and unstable character of meanings in the early stages of their development. Individual critical numbers and generalizations based upon them were given to slipping out of mind and to various distortions of meaning, which clearly reveal the weakness or utter lack of association with other elements of the situation. Thus Subject i, while working upon his seventh trial with 11 beads, appears to have had a fleeting insight into the significance of critical numbers as a means of actual control. Several other subjects spoke of a similar tendency for promising ideas to vanish during the attempt to formulate or apply them. Sometimes a critical number would be recognized and remembered well enough, but with an inversion of meaning somewhat comparable to the reversals of perspective in ambiguous drawings. Thus Subject xiv (11b, 29) said: "Whenever I win, I always have to leave 3 beads; that is, there must be 3 beads before I take my last draw." This sort of inversion of meaning often occurred shortly before the appearance of Stage E in the development of the concept of the critical number. After trying for some time at this point to get to draw from 6 beads subject viii said: "I forgot I was trying to keep from getting 6, and consequently for a good while I was trying to get 6." Another instance is found in the comments of Subject i quoted above (13b, 1). Similarly the idea of the significance of multiples of 3 often dawned and was lost, later to reappear, perhaps several times, before finally being applied in a thoroughgoing way with a full realization of its significance. Examples of the early instability of this idea are found in the comments listed above as follows: vii, 18b-22b; xii, 9b-30b, and xiii, 15b-27b. In tracing the further consolidation of the various elements of the concept of the critical number through succeeding series of problems, it is found that they are often thrown out of co-ordination by the appearance of some new element which requires adjustment and further generalization, or by the recall of some old hypothesis which has not been satisfactorily disposed of.

---

[29] For example, see the foregoing comments under Subject i, 12b, 3; Subject vii, 12b, 26; and Subject xiii, 12b, 36.

[30] Series in which the value of L is 1.

[31] Series in which all numbers between L and H may be drawn.

The interference of old and erroneous hypotheses with the formation and stability of correct generalizations, is worthy of further attention. The comments of Subject xiii, quoted above (27b, 2), show how the idea that odd and even numbers were possessed of special significance, came in to confuse him after he had made a perfectly good generalization. This subject had played somewhat with the idea of odd and even numbers much earlier in the series, but during the greater portion of the work he seems entirely to have dismissed the idea, making no mention of it at all. The very common interference of erroneous hypotheses and irrelevant ideas which have once received attention without being definitely settled, will be further illustrated in the following paragraphs on trial and error in generalization.

3. *Random Hypotheses*

Having observed the slow growth of meanings and their instability in the early stages of development, we may turn briefly to the manner in which the elements of a situation were brought together and combined into adequate generalizations. It rarely happened in the early members of a group of series that the essential elements were directly sought out and held in mind without attempts at generalization until sufficient data were at hand to insure correct inference at once. This type of procedure was followed apparently by one of our subjects in Series 1-2, but was not carried out consistently in all later series. The work of all other subjects in the first series showed some loss or omission of essential data with consequent erroneous generalizations. These random hypotheses were often numerous and far afield. The rate of progress depended very much upon the thoroughness with which they were followed up and tested. Because of the unusual fullness of his comments the work of Subject xiv will be taken to illustrate the random efforts at generalization in the first series. The place in the series at which each comment occurred will be indicated in the manner already familiar to the reader.

9b, 3: "You can always win if you draw the same as I."
11b, 20: "If, with an odd number, you take an even number, I can always beat you if I work it right."
" 36: "Whenever I get a combination by which I can win, thereafter I must reverse when you reverse in order to win."
15b, 6: "I win some odds and some evens."
" 7: "I draw another conclusion, that my principle is not to follow you."

" 8: "Neither is it to draw opposite from you."

" 9: "Neither is it to alternate with you."

17b, 1: "I can always beat you on even numbers; I don't think I can beat you on any odds."

" 9: "There must always be 3 beads when you draw last."

18b, 2: "As long as you draw opposite to me I can't win 18."

21b, 7: "You win odd numbers by taking the opposite of my draw."

22b, 2: "The principle of alternation will not work with the even numbers."

25b, 1: "Even and odd numbers is not the criterion."

" 12: "There must be some relation between the total number of beads and the number of draws."

" 13: "When I win, the critical draw is the second or no later." For some time after this the subject's attention was absorbed in the number of beads he obtained in each trial.

31b, 12: "I haven't kept track of the numbers I won and those I lost."

32b 1: "To win an *odd* number of beads I draw an odd number." Here again the subject's attention is absorbed for some time in the number of draws and of beads obtained in each trial.

34b, 21: "2 from 34 leaves 32. Now if I draw 1 always and you 2, I would leave even numbers." (!)

36b, 1: "It seems that I can't win every third time."

37b, 4: "Of course I know you are going to take 2 all the time and I'll take 1, but I don't know how it will come out."

40b, 1: "I will beat you but I can't say how with any degree of certainty. I'll beat you twice and then you will beat me once. I notice that the numbers upon which you beat me are always divisible by 3." The subject is asked how he would draw from 59 beads and replies that he cannot tell. He then figures slowly but correctly to 59, after which he is asked how he would draw from 1004 beads. "I couldn't figure that up," he said, "I may win even numbers and you odd numbers. No, I won 37. It may be numbers that are divisible by 2. No, I don't get it. . . . It might be that you allow me to beat you on all numbers that are divisible by a certain number, but it isn't by 2 and it isn't by 3 or 4 . . . I have never figured out this drawing business; I do it wholly according to my feeling. But I watch the way you draw on your second draw and then take the opposite. . . . I am not sure what is the criterion, whether it is your first two draws or my first two."

40b, 5: "I ought to have noticed that 3-matter long before because I noticed so often that we drew 1 and 2."

41b, 4: "I knew I could beat; I had 3 beads after my second draw."

42b, 1: "I can't beat you on 42, it is divisible by 3. I know what you can beat me on but I don't know why."

43b, 4: "They go in pairs. I can beat you on 43 if I take 2 the first time (He loses). You haven't arbitrarily made up your mind not to let me beat you the first time, have you?"

" 8: "When I win I guess I must draw 2 the first time; but I can't win that way, because I've tried it and failed. . . . But if you happen to be drawing the opposite of what I draw the second time, I'll beat you. . . . I'll take 2 and then you'll let me beat you."

" 9: "Well, I thought all I had to do to beat you the second time was to reverse what I did the first. I see, evidently I have to start out with the same number in order to beat you on a given number."

" 10: "Three from 43 leaves 40. All right, I have it. I have an *even* number; I'll take 2."

" 12: "I had 15 draws—Oh yes, that is all right. I was thinking I could get out by leaving an *odd* number of beads."

44b, 1: The subject takes 2 and wins. "That confirms my rule," he says.

45b, 0: "I can't win 45."

46b, 1: "I can win 46. I'll have to start with 2. Yes, I must or I can't win, I believe."

" 3: S draws 1 and wins. "On the numbers on which I can beat you," he says, "I have to start out with 1."

47b, 1: "I can beat you on this. . . . Well, that rule didn't work (i.e., that S must always draw 1 first to win). Well, maybe the rule is that one time I draw 1 in order to win and the next time I draw 2 to win."

48b, 0: "Did I win 47 by drawing 1? No, I won it by taking 2. Then I can't win 48, of course."

49b, 1: "I'll draw here on the hypothesis that on the first one after the number I lose, I can win by taking 1, and on the second after it I must start with 2 to win."

" 2: "It worked. *Now this time I'll start with 2* (S loses). No, that isn't right."

50b, 1: "Now I'll start with 2 and beat you." Here the subject is taken back to the beginning of the series.

7b, 1: (S takes 2 and loses) "Now that's funny."

" 2: "I'll take 1 to start with."

" 3: S takes 2 and loses. "I've got to start out with 1 on 7," he observes, "and there is a lot of other numbers on which I have found that I must start with 1."

8b, 1: "I'll try 2 just to see. No, I'll try 1."

" 3: "Why isn't the rule that on the *even* numbers I've got to start with 2?"

" 4: "Yes, that's the rule."

9b, 1: "Oh, I can't beat you on 9, and *on 10 I've got to start with 2.*"

10b, 2: "Oh! I mustn't take 2 the first time; I must take 1."

11b, 2: "I believe my hypothesis was right about there being first the 1-draw and then the 2-draw. On 4 I had to draw—I forget just how I did draw on 4. On 10 I drew 1 and on 11 I took 2. I think I must take 2 on 11."

12b, 0: "I can't win 12; it is a multiple of 3."

15b, 0: "I can't get it."

16b, 0: "Now I'll take 1."

17b, 1: "I'll draw 2." The subject was then asked how he would draw from 59 beads. "Why," he said, "it goes this way. I draw 1 first, then you draw 2 and I keep on with 1. Then I beat you. The next time[32] I start with 2 and you start with 1, and I keep on with 2 and win. But I can't tell how a given number must be drawn on. I just know that it goes along 1 and 2. What is this number? (17) Well, on 17 I had to draw 2 didn't I? Well, I should guess that on 59 I should start with 2, and then you would draw 1 and I would continue with 2. (Why?) The only reason I say that is that it is just before 60, and 60 is a number that you beat me on just as you do on 18. Seventeen is just before 18 and 59 is just before 60." In reply to a question as to how he would draw from 43 beads the subject said: "I would draw 1 on 43, because on 42 I couldn't beat you, and with the numbers following those on which I can't beat you I must always start out on 1."

The random making of hypotheses upon the basis of momentary suggestions is here too evident to require further comment. Examples might be produced almost indefinitely from Series 1-2. A few examples from higher series and from the efforts of subjects to get a generalization of universal application for all series will perhaps be worth while.

The following evidences of random hypothesis-making are taken from the record of Subject viii. In attacking Series 2-3 this subject fluctuated a good deal among a number of hypotheses, as is shown in his comments:

7b, 2: "I think now that I will win on the basis of 7, i.e., I will try always to draw so that there will be 7 left to draw from."

8b, 2: "It appears that I can win on a basis of 5."

9b, 2: "It appears that 7 doesn't work."

" 3: "It doesn't work. I forgot about the 1."

" 6: "It will work on a basis of 6 as well as of 5, i.e., on multiples of both 3 and 2, I believe. But I am not certain."

---

[32] Note the possibility of confusion in the phrase, "*next time*." It may mean the next trial upon the same number or it may mean the next higher number. These two meanings fluctuate in the mind of the subject, owing perhaps to the ambiguous wording. The same ambiguous wording appears in the comments of this subject at 47b, 1 (p. 94), though here it is clear that the subject has the correct idea in mind. But it is the wording rather than the idea, that functions in the immediately following problems (47b, 1 to 7b, 3) in such a manner as to produce a number of errors. Several other instances were found of ambiguous wording which led other subjects into confusion and error.

15b, 3: "I am a little in doubt about the multiple of 3 and 2. It doesn't seem to work."

20b, 3: "Twenty can't be worked. I didn't think of that multiple until I had worked for a long time. It is a multiple of 5."

21b, 1: "Sometimes the multiple of 3 works and sometimes it doesn't. Either a multiple of 2 or a multiple of 3."

21b, 5: "At first I thought I could work on the basis of 2 or 3, but one of the multiples of 3 didn't come out right. I almost think now that I made a mistake and that I can't work on this basis."

26b, 2: "That hypothesis couldn't have been right because 12 is a multiple of both 2 and 3 (S won 12 repeatedly), so I have given that up."

The subject was tormented by these and numerous other hypotheses until he had solved all of the numbers of this series from 6 to 51 and then repeated the process up to 29 beads.

Perhaps the most striking exhibition of this sort of random effort was found in the attempts of subjects to formulate a general solution for all series. Here it was usually necessary to give them a list of the critical numbers of all discontinuous series in order to refresh their memories.[38] All of the subjects had made some discoveries, as for example, that all multiples of L + H are critical numbers. The generality of some of these earlier formulatonis was readily recognized if not already known. But some were not universal in their application, and here is where the effort began to be more sporadic. The subject would usually begin by restating one of his old generalizations or by formulating a new one on the basis of one or two problems, and then go from series to series testing it out until a series was discovered to which it would not apply. Upon finding a refractory series he would stop and attempt to modify his view to suit the case in hand and then pass on to test it out upon other series. Often the subject would have to modify his generalization for every group of series which were in any marked degree different from the preceding ones, sometimes discarding the notion only to return to it later in his fumbling efforts to find a generalization which would be applicable to all series. Some subjects failed after working for hours, and most of the others succeeded in getting a general solution only by a tedious "fitting on" of numerous hypotheses suggested by individual series and often wholly at variance with conditions which had been met repeatedly in other series. A better exhibition of random effort could not have been given by an animal in a problem box.

---

[38] Series in which only L and H may be drawn. For a list of these similar to that presented to the subjects see page 7.

Nor is it to be supposed that in the application of generalizations to new situations we have an entire escape from trial and error procedure. Numerous errors and omissions of application point to the random character of efforts to apply generalizations. Certain hindrances to application and some sources of error are fairly apparent in the transition from the continuous to the discontinuous series of problems and in passing from the first to the second series of the latter group.

Three of the more important elements of their previous generalizations which subjects brought over into Series 1 or 3 are (1) the critical-number concept, (2) the idea of the serial relation of the critical numbers of the series, and (3) the knowledge of the fact that the common difference between successive terms of the various series of critical numbers is equal to L + H. No difficulty was experienced in applying (1) and (2) directly to Series 1 or 3. The trouble with the application of (3) arises from the fact that a new series of critical numbers is created by the restriction of the draws to L and H. The first term of this new series is 2 and the common difference is L + H as before. This new series of critical numbers should have given but little difficulty if the subjects had stuck to the old generalization for *primary* critical numbers and looked for an explanation for the *secondary*[34] critical numbers only. But there were too many pitfalls in the way. In the first place, the first critical number encountered was a secondary one. This refractory case immediately cast doubt upon the applicability of the old formula and lessened its chances of later consideration. Again, the secondary critical numbers fall into line with the primary ones in such a manner as to form a continuous series with a common difference of $\frac{L + H}{2}$. This is, of course, a coincidence peculiar to this series and not common to all. Finally, the common difference of $\frac{L + H}{2}$ is 2, in this particular case, making all *even* numbers critical. Thus the mind of the subject is led unawares away from the general principle built up in previous series and directly into the rut of the old concepts of even and odd numbers. The power of these superficial relations and the familiar concepts aroused by them to divert the attention of subjects from the application of generalizations formed in previous series, is evident from the fact that twelve of the thirteen subjects who solved this series, stated their final formulae in terms of even numbers, though five of them had played with the old formula while working upon the early problems of the series.

---

[34] See above, p. 8 for definition of these terms.

Some interesting errors of application were made by Subject viii in passing from Series 1 or 3 to Series 1 or 5. After having generalized correctly for Series 1 or 3 he began his attack upon the following series correctly enough by saying: "I would think right away that 6 is the lowest critical number and that all multiples of 6 are critical." But he quickly got off the track and began to believe that all multiples of 3 were critical. Later he supposed that the critical numbers were all multiples of 9, but finally changed his conjecture to muitiples of 12. After obtaining a solution for this series he stated that the errors had arisen from his efforts to adapt the generalization of the preceding series—i.e., that "All multiples of 2 are critical"—to the present one. Examining the former series to find the basis of the distribution of critical numbers, he observed that 2 lies midway between 1 and 3, the L- and H-draws respectively, and straightway inferred that multiples of 3 were critical in Series 1 or 5 because 3 lies midway between the L- and the H-draw in this series. Finding this hypothesis to be incorrect he tried the sum of all the numbers between 1 and 5, i.e., 9, because it was observed that "2 constitutes all the numbers between 1 and 3. This failing, he "thought that a critical number had to be a multiple of all of the numbers between 1 and 5, i.e., a multiple of 12.

One of these errors was repeated by this subject in passing from Series 2 or 6 to Series 2 or 10. In generalizing upon the former series he said: "Since about the beginning of the last series I have felt that it must go by multiples of 4, because 4 is intermediate between 2 and 6 and is a factor of both." Immediately afterward, when presented with Series 2 or 10, the subject said without hesitation: "I should say the critical numbers are multiples of 6 and numbers which are 1 above those multiples; that is, I add 2 to 10 making 12 which I know is a critical number. Then I reduce to 6—No, I didn't, did I?—as I had, in the other case, reduced 8 to 4."

In generalizing upon Series 3 or 9 Subject ii said: "The critical numbers are multiples of the difference between the high and the low draw." When presented with the next series, he said: "Immediately I take the difference between 4 and 12, which is 8. This formulation applies very well to all series in which H = 3L, but to no others. Although the subject did not explicitly say so, he undoubtedly carried this generalization over into Series 1 or 4 and Series 1 or 6 much to the detriment of his progress. While working upon 9 beads in Series 1 or 4 he decided that 3 was a critical number and said: "I feel that I am on the edge of something here . . . 3 I lose. It reduces to 3." Shortly afterwards he added: "I've been mistaken there. I lose 3 and 5. No, I win 3." After thinking this matter over for two minutes he continued: "I believe I *can* win 3, 6, and 9." A little later, after a short pause to determine how to draw from 9 beads, the subject said: "Now that's funny. I can *win* 9." The idea that multiples of 3 are critical numbers in this series seems finally to have been discarded at this point, but the generalization upon which it rested,—i.e., that the difference between successive critical numbers is equal to H — L,—persisted into the next series and seems to have been the cause of considerable difficulty there. The idea, carried over from Series 1 or 3, 1 or 5, and 1 or 7, that *even* numbers are critical, is also much in evidence in the records of Series 1 or 4 and 1 or 6. The ill effect of these generalizations from earlier series upon the progress of the subject through Series 1 or 4 and 1 or 6, is evident in his records. Though in the speed of progress throughout the entire experiment this subject ranks second, he is tenth in rank in Series 1 or 4 and 1 or 6 combined. The other nine of the ten subjects who solved all of the series devoted only 10 per cent of all their trials to these two series, whereas Subject ii devoted 32 per cent of all his trials in the entire experiment to the solution of them. It appears, therefore, that approximately two-thirds of the difficulty encountered by Subject ii in these two series, was due to the interference of certain generalizations carried over from preceding series.[85]

Many other examples of the difficulties attending the application of generalizations could be given but the principal types are perhaps sufficiently illustrated in the foregoing cases. The omissions and errors of application occurring in the records of all subjects are numerous enough to warrant the statement that, under the conditions of our experiment at least, the application of generalizations depends very much upon trial and error procedure. It appears that any incompleteness of analysis, either in the old situation from which the generalization is evolved or in the new situation to which it is to be applied, is likely to render application difficult and perhaps erroneous. There is no apparent reason why difficulties of this nature should arise from the conditions of our experiment more readily than from the practical situations of life.

4. *Summary*

Explicit generalization did not often occur until the number of beads presented was high enough to preclude the possibility of direct, perceptual foresight of the consequences of all possible draws, i.e., until the numbers presented were high enough to place a premium on the use of symbols in their solution. Numbers lower than this were seldom taken into account even in later attempts at generalization.

---

[85] It is of course possible that other factors contributed to this retardation of progress. Fatigue may have affected the progress in Series 1 or 4. This series was completed shortly before noon when the experiment had been in progress approximately three hours. The subject stated that he was becoming somewhat fatigued though not until after much difficulty had been encountered. However, the greater portion of the effort was expended upon Series 1 or 6 where fatigue could not have been a factor. This series was begun after an intermission of one and a half hours, when the subject declared that he was thoroughly rested.

Several fairly distinct stages in the development of the critical-number concept were found. Each advance to a higher level of meaning began usually with that critical number, above the range of perceptual solution, which had been reacted to most frequently, and affected the higher critical numbers largely in the order of the frequency of their repetition. This is also the order of their numerical and temporal nearness to the goal, i.e., the end of the trial. In the early stages of their development the meanings involved in the concept of the critical number were found to be extremely unstable in character. The stability of these meanings gradually increased with continued reaction to the situations from which they were evolving.

The selection of the essential elements of a series and their combination into adequate generalizations was effected mainly by means of trial inferences, or random hypotheses, which were made upon the basis of only a few cases—often only one—and tested out more or less persistently before final acceptance or rejection by the subject.

In the application of generalizations to situations which are in part new, much difficulty was experienced, and a considerable amount of random modification often occurred before a successful adjustment to the requirements of the new situation could be made. False analogies arising from the observation of superficial relations often resulted in confusion and error. Sometimes the solution of new problems was much retarded by the attempted application of inadequate or irrelevant generalizations formulated from the elements of earlier series.

F. TRANSFER

The term, *transfer,* is used by Ruger broadly "to include the effect of any given experience on any subsequent one whether the effect results directly or by means of an idea, whether the transfer is one of method or of material, or of motor processes, and whether it is positive or negative."[86] The term will be used in the same broad sense in this discussion. This usage is becoming fairly common. Ruger's defense of it need not detain us.

---

[86] *Op. cit.,* p. 85.

1. *Degree of Transfer*

Time and other limitations have not permitted such experimental evaluation of the various problems as would be required for a highly accurate measurement of the transfer of the effects of learning from problem to problem. However, the facilitating and, sometimes, the inhibiting effects of earlier upon later efforts, are great enough to give a fair insight into the causes and conditions of transfer without a very precise determination of its amount. A rough estimate of the relative difficulty of various problems and series of problems can be made from some of the experimental data and other facts.

Within a given series the higher problems are undoubtedly more difficult than the lower ones. If the subject's draws were determined by pure chance, two successive winnings would require 8 trials upon 5 beads, 32 trials upon 8 beads, 128 upon 11 beads, and 512 upon 14 beads. This increasing difficulty is sufficient to conceal the effects of transfer in many instances, especially in the lower numbers of the series. Though no experimental evaluation of the various problems of any series was attempted, the records of Group III when compared with those of Group IIA show that the solution of 14 beads was considerably more difficult than that of lower numbers of the series. Group IIA began with 4 beads and followed the same procedure as Group I. Group III followed the same procedure but began with 14 beads instead of 4. The number of trials required by each member of this group for the solution of 14 together with some data for comparison from Group IIA, are given in Table XII.

The subjects of Group III required a greater number of trials for the solution of 14 beads than were required by those of Group IIA for the solution of the first seven problems of the series, and over half as many as were required by the latter group for the solution of the first ten problems. Assuming that the two groups were possessed of equal ability to solve problems

TABLE XII

Group IIA					
	Total No. of trials required for the solution of all problems from 4 to 10 inclusive	Total No. of trials required for solution of all problems from 4 to 13 inclusive	No. of trials required for solution of 14 beads		
			Group IIA	Group III	
Subj. A	32	48	0	Subj. 1	8
" B	39	49	5	" 2	11
" C	25	68	2	" 3	15
" D	56	88	4	" 4	27
" E	34	106	4	" 5	29
" F	59	84	4	" 6	35
" G	36	112	22	" 7	42
" H	74	97	25	" 8	50
" I	47	159	2	" 9	60
" J	39	92	32	" 10	63
				" 11	86
				" 12	180
Total	441	903	100		606
Average	44.1	90.3	10.0		50.6

of this sort, the transfer from an average of 90.3 trials upon the first ten problems to the eleventh problem of the series was equal to 40.6 trials, or to 80.2 per cent of the average number of trials required for the solution of 14 beads when presented as the initial problem. Further profitable discussion of transfer from problem to problem within a series must await a careful determination of the difficulty of these problems.

From logical analysis it would seem that all series of the same order are of approximately equal difficulty and that series of higher orders are somewhat more difficult than those of lower orders.[37] Some experimental evidence of the substantial equality of difficulty of Series 1-2 and Series 1-3, is found in the records of Groups IIA and IIB, which are summarized in Table XIII.[38] The subjects are listed at the left in the first column. The number of problems solved by each subject in each of the series and the number of trials and amount of time required for the solution of the series, are given in the succeeding columns.

---

[37] In Series 1-2 there are just four combinations of draws by which subjects can win non-critical numbers. Upon problems in which the initial number of beads is greater by 1 than a multiple of H + L the subject can win half of the trials by taking 1 bead at every draw. The other half he can win by taking 1 at his first draw and 2 at every draw thereafter throughout the trial. He can win one half of the trials upon problems in which the initial number is less by 1 than a multiple of H + L, by taking 2 beads at every draw. The other half of these problems he can win by taking 2 at his first draw and 1 thereafter throughout the trial. If L and H be substi-

tuted for 1 and 2, these four combinations of draws are effective for the solution of some problems in every series of the first order. But for all problems in which the initial number is removed from a multiple of H + L by an interval greater than 1, a new combination of draws must be made by varying the first draw. In Series 1-3 a new combination is required for only those problems in which the initial number of beads is an odd multiple of 2; i.e., for one-third of the non-critical numbers of the series. In Series 1-4 it is required for half and in Series 1-5 for three-fifths of the non-critical numbers, etc.

Moreover, the discovery of the successful combinations of draws is not favored at so early a point in the higher as in the lower series, owing to the magnitude of the H-draws which effect a reduction of small numbers to 0 before a sufficient number of repetitions has been made to impress the uniformity of response upon the mind of the subject. For the same reason the early discovery of the principle of drawing by opposites is less likely in the higher than in the lower series. Again, because of the longer interval between critical numbers in the higher series, a greater number of problems must be solved in these series than in lower ones in order to reveal a given number of critical numbers upon which to base and with which to test generalizations.

On the other hand, each critical number in the higher series is more firmly fixed in mind than those of the lower series by the more frequent reaction of subjects to it before the next higher critical number is presented. This would favor the more ready utilization of critical numbers in the higher series for purposes of generalization and so facilitate the solution of these series. This higher degree of learning is the only factor revealed by analysis which clearly favors the more rapid solution of higher than of lower series of the same order. How important these various factors are cannot be stated definitely without further experimentation. It does not seem unreasonable, however, on the basis of this analysis, to assume that the higher series are at least as difficult as lower series of the same order.

Critical numbers are relatively more numerous in higher than in lower orders of series, and their relations to one another are more complex. It is practically certain, therefore, that a series of a higher order is more difficult than one of a lower order.

[38] The subjects of these groups were members of a class in educational psychology, whose class work had been observed by the writer for from three to seven months. The division into groups was made upon the basis of reasoning ability as judged by the writer.

Table XIII

Group IIA

	Series 1-2			Series 1-3		
Subj.	No. Problems	Trials	Time (in seconds)	No. Problems	Trials	Time
A	10	48	1833	12	37	1130
B	15	62	1555	8	18	230
C	15	85	1840	7	13	248
D	16	112	4210	6	24	685
E	21	220	6450	7	15	130
F	74	300	19133	9	31	1545
G	27	332	7440	0	0	0
H	76	389	13663	8	16	312
I	81	617	15988	8	17	162
J	162	823	18572	88	524	10260
Total	497	2988	90684	153	695	14702
Average	49.7	298.8	9068.4	15.3	69.5	1470.2

Group IIB

	Series 1-3			Series 1-2		
Subj.	No. Problems	Trials	Time (in seconds)	No. Problems	Trials	Time
a	16	48	2545	9	14	300
b	16	50	2062	12	29	763
c	19	56	4545	9	15	407
d	16	69	2640	4	10	135
e	17	95	2090	3	7	70
f	23	179	9585	1	2	120
g	38	210	8455	6	14	270
h	26	337	10131	12	31	532
i	52	376	18648	3	6	150
j	98	776	20435	12	30	1054
Total	321	2196	81136	71	158	3801
Average	32.1	219.6	8113.6	7.1	15.8	380.1

The average number of trials per subject in Series 1-2 is 298.8, P.E., 51.9. In Series 1-3 the average number of trials per subject is 219.6, P.E., 46.3. The difference between these averages is 79.2, P.E., 69.5. In so far, therefore, as the calculation of unreliability means anything when based upon so few and so variable data as those at our command, these data support the view that there is no substantial difference in the difficulty of different series of the same order.

In terms of the number of trials required to solve the series, the transfer from Series 1-2 to Series 1-3 is 76.7 per cent. From Series 1-3 to Series 1-2 it is 92.8 per cent. The lower percentage of transfer in the former case is due almost wholly to the behavior of Subject J. The insight of this subject into the first series at the time of her successful generalization was unusually superficial and, contrary to custom, five days were permitted to pass between her completion of the first series and her attack upon the second. If we leave her record out of account, the degree of transfer for the group becomes 92.1 per cent. The transfer from either series to the other is here so great as completely to overshadow such differences as may exist in the difficulty of the two series.

The degree of transfer from series to series within the various orders of continuous series is shown in Table XIV. The percentages of transfer are based on the supposition that all series in any given order are equal in difficulty. The series are listed at the left of the table in the usual order. Individual subjects of the group are indicated by the Roman numerals at the top. Though not all of the series were specifically solved by all subjects the general solutions given for all series of an order were such as to indicate that all higher series of the order could be solved without manipulation of the beads. We have therefore figured the average number of trials per series just as if every series had been specifically solved by every subject.

The degree of transfer may be further traced from lower to higher orders of series. As already stated, the higher orders are probably more difficult than the lower ones, but in the absence of experimental data upon this point we shall assume that all orders are of equal difficulty and calculate the percentages of transfer upon that basis. The transfer from lower to higher orders, as found in the records of the thirteen subjects who solved all of the continuous series, is shown in Table XV.

Further evidence of transfer is found in the constant decrease, through successively higher orders, in the number of series required to be solved for the mastery of an entire order. The average numbers of series solved by actual manipulation of the

TABLE XIV

Transfer from Series to Series, Group I

Series	Subj. i	ii	iii	iv	v	vi	vii	viii	ix	x	xi	xii	xiii	xiv	Ave.	Per cent of trials saved as compared with first series of same order	Per cent of transfer from next preceding series
1-2	48	35	28	96	62	331	371	168	315	597	53	184	166	367	201.5		
1-3	7	23	10	35	15	30	23	56	16	45	34	11	20	1	23.3	88.4	88.4
1-4	14	0	0	27	3	4	8	30	3	3	0	16	35	0	10.2	94.9	56.2
1-5	0	0	0	10	0	0	0	2	2	0	0	0	18	0	2.3	98.8	77.4
1-6	0	0	0	0	0	0	0	0	9	0	0	0	16	0	1.8	99.1	21.7
1-7	0	0	0	0	0	0	0	0	0	0	0	0	19	0	1.4	99.3	22.2
1-8	0	0	0	0	0	0	0	0	0	0	0	0	2	0	.1	99.9	89.5
2-3	44	17	25	44	71	30	25	167	120	65	117	89	40	43	64.1		
2-4	0	4	84	0	29	1	13	71	54	26	21	45	8	115	33.6	47.6	47.6
2-5	0	0	60	0	0	0	0	20	15	2	21	1	0		9.2	85.6	72.3
2-6	0	0	0	0	0	0	0	2	0	0	14	0	0		1.2	98.1	86.9
3-4	0	0	0	0	0	1	12	41	10	40	20	3	16		11.0		
3-5	0	0	0	0	0	0	0	4	9	0	13	0	70		7.4	34.5	34.5
3-6	0	0	0	0	0	0	30	0	0	12	0	0	0		3.2	70.9	56.8
4-5	0	0	0	0	0	0	0	0	0	10	29	0	31		5.4		

TABLE XV

Transfer from lower to higher Orders of Series

Order	Ave. No. of trials per subject	Per cent of trials saved as compared with first order	Percentage of transfer from preceding order
First	230.8		
Second	103.5	55.3	55.3
Third	21.6	90.6	79.2
Fourth	5.4	97.7	75.1

beads in the various orders of continuous series were 3.38, 2.46, 1.08, and .23 for the first, second, third, and fourth orders respectively.

Owing to various irregularities which make their comparability uncertain, no attempt will be made to determine the degree of transfer in the discontinuous series. That much positive transfer occurred can readily be seen by referring to the data upon these series in Table III.

2. *Conditions of Transfer*

No attempt will be made to enumerate all of the conditions and sources of transfer but a few of the more important ones will be mentioned and discussed briefly.

a. Objectively Identical Elements.—In order to solve any of the higher problems of a series it is necessary to make all of the moves appropriate to the solution of all lower non-critical numbers of the series. All of the lower numbers of a series are therefore common elements of the higher problems of the series. Thus in Series 1-2 the numbers 4, 5, 6, 7, 8, etc. must all be solved in the process of solving the higher numbers of the series. That the solution of these lower numbers contributes to the mastery of higher ones is shown by the results reported in Table XII. Also the effect of the presence of common critical numbers is shown by the fact that subjects soon learned the necessity of avoiding certain critical numbers, and later of forcing the experimenter to draw from these numbers regardless of what might be the initial number of beads presented for solution.

Likewise in all series of a given order the lowest possible draw is a common element; e.g., in Series 1-2, 1-3, 1-4, etc. the low draw is the same for all series. It is clear that this common element is responsible for a large portion of the transfer, for after the solution of Series 1-2, in which neither the high nor the low draw was usually recognized as especially related to the formula, it required only 41.7 trials on the average to generalize for all possible variations of the high draw with the low draw remaining constant. But it required 130.3 additional trials on the average to generalize for all possible variations of both the high and the low draw.[89] In other words, the mastery of an indeterminate number of series in which one of the two most essential elements is identical, required less than one-third as many trials as were necessary for the mastery of an indeterminate number of series in which this objectively identical element is lacking. And this notwithstanding a large amount of transfer through other elements from the former to the latter group of series. Other common elements of this sort might be mentioned, as for example, the different sequences of E's draws (described above, p. 18), but with further enumeration it becomes increasingly more difficult to distinguish between subjective and objective identities.

It must not be supposed that these objectively identical elements were invariably recognized at once as common to the various problems of the series. Even the most obvious of them were often very slow in impressing themselves upon the minds of some subjects. But the absence of explicit awareness of these elements is no warrant for the contention that no transfer was mediated by them in the early stages of the experiment. The influence of unrecognized factors of some sort is clearly evidenced by the fact that subjects were frequently able to solve problems though unable to say how it was done or to give better reasons for correct draws than that they "seemed right."

b. Subjectively Identical Elements.—Many of the more important elements which came to be regarded as common to a number of problems are not, however, present in the same objective sense as those mentioned above. Such identical elements are, in large measure, the result of the generalizing activities of the subject. Judd has insisted at length upon the importance of this type of common element as a source of transfer.[40] Elements of this sort are fairly numerous and constitute the principal medium of transfer as found in this study. The high draw, or H, for example, is clearly dependent upon generalization for its status as a common element in all series, as is also the low draw, or L. These two symbols with their associated meanings are undoubtedly the most effective instruments of transfer for all subjects who mastered any considerable number of series. It is through the medium of these two minor concepts, largely, that the meanings developed in and about specific critical numbers, become detached and generalized and finally applied to other appropriate elements in the same and other series. On the basis of these and other generalized elements every subject, who had the perseverance to stay with the task, sooner or later acquired the ability to solve at sight series in which neither the low nor the high draw was common to the preceding series in a purely objective sense.

---

[89] The intermediate draws may be neglected since they varied in the same manner in both cases and generally attracted little or no attention.

Common elements of these two types were not always easy to distinguish, however. Specific elements of the objective sort were often apprehended in general terms at their first presentation, or, at any rate, the subjects' verbal reactions to them were couched in general terms. Thus the fact that, after reducing the number of beads to a multiple of L + H, E always drew 1 when S drew 2 and 2 when S drew 1, was often noticed and alluded to by the subject as "drawing opposites."

c. Generalized Methods of Procedure.—Closely related to the generalized elements of content and depending largely upon them were certain generalized methods of procedure. The most common of these types of procedure to develop in the first series and persist through later series, consisted of some sort of systematic alternation of various sequences of draws designed to prevent useless repetition of unsuccessful variations or to exhaust the possibilities of variation. One subject described his procedure as follows: "First I take 1 and then the same as you took (for the remaining draws of the trial), then 1 and the opposite; then I take 2 and the same, and finally 2 and the opposite." The first and third of these sequences of draws were not successful. The others were successful approximately half of the time upon non-critical numbers.

---

[40] Judd, Psychology of High School Subjects, p. 414.

There is good evidence that the erroneous sequences were brought in by a mere contrast association between the words *opposite* and *same*. Here are the circumstances: After winning twice in succession from 14 beads, the subject said: "If I take 1 when you take 2 and 2 when you take 1, I win; i.e., if I take the *opposite* of your draw." After three trials upon 15 beads he said: "The principle I discovered seems to hold now for you." He then began immediately to follow the experimenter's draws, i.e., to draw the *same*. This failing in the first three trials, he changed again to the opposite in the fourth trial, and added at its completion: "When that was reduced to 12, I tried taking the opposite of what you took, but it didn't work." He then began to follow the procedure described above and continued for over an hour to the very obvious detriment of his progress. Finally the erroneous sequences were eliminated and his progress somewhat accelerated. This modified procedure was carried forward to later series.

Another helpful procedure which was used quite effectively by some of the subjects was to begin the search for critical numbers with 0 which was regarded as the first critical number in every series. This had the effect of limiting the number of problems to be solved in locating a sufficient number of critical points for generalization. The procedure was adopted by Subject vi at the beginning of Series 1 or 6 and by Subject ii near the beginning of the experiment.

An effective method of limiting the scope of attention in the discontinuous series without sacrificing any useful variations, was to subtract the highest multiple of L + H contained in the number presented for solution, and work upon the remainder which was thus brought well within the range of perceptual control. In Series 2 or 5, for example, if 17 beads were presented. the subject would find the difference between 14 and 17 and deal directly with this small remainder. After the first draw had been determined in this manner it was usually possible to make the remaining draws mechanically upon the principle of drawing opposites. This procedure was followed more or less persistently with good effect by about a third of the subjects.

A less definite though perhaps more profitable form of systematization consisted in a better balancing and correlation of rational thinking with overt trial-and-error procedure as the experiment progressed. In the early problems much of the manipulation was extremely ill-directed, or undirected, and much time was wasted in fruitless attempts to generalize upon insufficient data. The later work of most subjects showed much improvement both in the purposeful direction of trial-and-error procedure and in the matter of judgment as to when enough data were in to warrant generalization. This process of empirically working out a proper balance and correlation between reflective thinking and overt trial and error, is probably one of the main sources of progress in all successful attempts to deal at all extensively with novel data.

d. Effect of Thoroughness of Learning upon Transfer.—It might be expected that the superior ability which enabled one subject to outrank another in the mastery of Series 1-2 would give him a proportionate advantage in the solution of the later series. As a matter of fact, however, subjects whose speed of progress in the solution of Series 1-2 is above the median show no superiority in the immediately following series over those whose speed is below median in the first series. This is true of all groups of subjects who solved as much as two successive series of problems, as is shown in the following data. The rate of progress is expressed in terms of the number of trials required for the solution of a series:

Although the subjects whose rate of progress was below median required over five times as many trials for the solution of the first series as was required by those whose rate was above median, they were able to solve the second series in practically the same number of trials as was required by their presumably more gifted fellow subjects. The degree of transfer from the

	Group I (14 subjects)		Group IIA[41] (7 subjects)		Group IIB (10 subjects)	
	First series	Second series	First series	Second series	First series	Second series
Total trials of subjects above median[42]	488	144	307	92	318	75
Total trials of subjects below median	2333	182	1638	64	1878	83

	First series	Second series
Total trials required by 16 subjects above median,	1113	311
Total trials required by 16 subjects below median,	5849	329

first to the second series, in terms of the number of trials required, was on the average 94 per cent for the subjects whose rate of progress in the first series was below median and only 72 per cent for those whose rate was above median. That these group averages do not grossly misrepresent the individual records is evident from the fact that only 4 of the 16 subjects whose rate of progress was below median in their respective groups in the first series, were surpassed in the percentage of transfer by any of the 16 subjects whose rate was above median. Correlations by the rank method between the quickness of mastery of the first series, as measured by the number of trials required, and the degree of transfer from the first to the second series, give coefficients of —.803, —.914, and —.819 for Groups I, IIA, and IIB respectively.

The thorough acquaintance with the elements of the problems, which is acquired by the initially slow subjects during their long-continued efforts to find a solution, is undoubtedly the main cause of the relatively high degree of transfer found at this stage of the work of these subjects. The records show clearly that the initially slow subjects generally surpassed the more speedy ones in the number and variety of elements which were discovered in the first series. From their four-fold more numerous responses to the same objective situations, it is not unreasonable to suppose that many of the associations formed by the slower subjects were more firmly fixed than corresponding associations formed by the subjects whose progress was more rapid. The difference between the subjects whose initial progress was slow and those who progressed rapidly at the start, is not so much a matter of difference in the speed of formation of new associations as in the power to utilize these associations for a definite purpose. But the ready utilization of associations in the early stages of their development results in the accomplishment of the end before a sufficient number of repetitions has been made to fix the associations thoroughly. It also limits the number and variety of associations formed. Therefore the points of contact are usually neither so intimate nor so numerous, and transfer at this point is generally not so great, when the initial progress is rapid as when it is slow.

---

[41] The record of Subject J is here omitted because of an irregularity of procedure mentioned above on page 110.

[42] Above median here means above median speed of progress as in the foregoing text.

3. *Negative Transfer*

Numerous facilitating and inhibiting factors enter in at every stage of the work to determine the rate of progress of subjects. On the whole, the facilitating influences so far outweigh the inhibiting ones as generally to make it difficult to detect the presence of the latter, to say nothing of the accurate measurement of their effects. Occasionally, however, the presence of these negative factors becomes clearly evident from the comments of subjects, and sometimes their effects become so obvious as to make a rough quantitative statement of their strength possible. Thus it has been shown above (pp. 80-81) that the progress of Subject ii through Series 1 or 4 and 1 or 6 was only approximately one-third as rapid as might reasonably have been expected but for the interference of certain superficial associations which were formed in the process of solving a few of the preceding series. Two clear cases of negative transfer were also given above (pp. 79-80) from the record of Subject viii.

A very similar case of negative transfer from Series 1-2 to Series 1-3 is found in the record of Subject ii. In his generalization upon the data of the former series he said: "I can't win any multiple of 3." When presented with the first problem on Series 1-3 he said at once: "I suspect right away that it will be a multiple of 5 here—3 plus 2 equals 5." After his first trial, however, he said: "I was mistaken. I see now that 3 plus 2 plus 1 equals 6. Evidently I *can* win multiples of 5. It must be multiples of 6." This idea was wrongly confirmed by his first trial upon 6 which was a failure. His pleasure at the seeming confirmation of his theory was evident from the tone of his remarks. "By Jove," he said, "you won that!" The persistence of this hypothesis is shown in his later attempt to account for his inability to win 8 beads. "If I take 1," he said, "you might take 1 and leave 6, and I can't win 6, so I can't win 8." The persistence of this notion is again apparent in his final attempt to generalize at the conclusion of the series: "I add the numbers which one is permitted to take and find that the multiples of the resulting sum cannot be won." This significant slip of the tongue was quickly corrected, however, as follows: "No! No! I add the last number to the first because that would give you the total number which could be taken with two draws."

If the first two series are equal in difficulty, the algebraic sum of all positive and negative transfer effects was only 31.4 per cent for this subject, though the average for all other subjects amounted to 89.1 per cent, and for the other six above-median subjects of Group I, 73.3 per cent. If the ratio of the number of draws required for the solution of the second series to those required for the first series had been the same as for all other subjects combined, he would have solved Series 1-2 in 4 trials. If this ratio had been the same for him as for the other six above-median subjects of Group I, he would have solved the series in 10 trials. Thus, figuring on a conservative basis, the negative transfer occasioned by the false analogy between the two series, and perhaps by some other inhibiting factors, was equal to 13 trials, or more than one-third the number required for the solution of the first series, and more than half the number required for the second series.

At the conclusion of Series 1-3 Subject iii generalized for all series as follows: "In general, you have got to add 1 to the highest number you may draw, and multiples of that sum cannot be won, e.g., for Series 1-3 it would be multiples of 4, for Series

1-4 it would be 5." Later in generalizing for Series 2-3 he said: "Combinations of 5 *and 1 above* are losers, i.e., numbers ending in 1 or 6." In Series 2-4 this subject did very well at the beginning. At 10 beads he said: "I win by leaving 6." Later when working with 11 beads he reiterated in more general terms: "When the starter can leave 6 beads, he wins it." After two trials upon 12 beads he added: "Combinations of 6, 12, etc., if they can be left by the one who draws last, win; i.e., the player who can leave 6, 12, etc. wins. After the second trial upon 13 beads the subject generalized as follows: "Combinations of *1 above any number*[48] or *1 above* any combination lose." He was asked to give an example and replied: "Well, 4 plus 1 equals 5. Combinations of 5, 10, 15, etc.; or combinations of 6, as, 6, 12,—no, combinations of 5 and 1 above, as, 6, 11, 16, etc. . . . anything that you can take away enough from to leave 5 you win."

Note how neatly the idea, "Add 1 to the highest draw," slips in from the generalization for all series of the first order to vitiate his reasoning while he is attempting to apply the "one-above" element from the formula for Series 2-3. Such a confusion could never have occurred if in the first formula "Add 1 to the highest draw" had been generalized into the more widely applicable form, "Add L to the highest draw"; that is to say, if the 1 to be added to the highest draw had been properly associated with the lowest possible draw in each series. Such association would certainly have prevented the addition of 1 to 4 in Series 2-4 to find the number whose multiples are critical numbers. The erroneous application of this idea is perhaps due in part to the similarity of its wording to that of the "one-above" element of the formulation for Series 2-3, for it was during the attempt to apply the latter element to Series 2-4 that the former one insinuated itself into the process. In the last instance, it will be observed, the wording of the two ideas is exactly the same, i.e., "*1 above* any number" and "*1 above* any combination."

---

[48] Probably meaning 1 above the high draw in any series, since he had already generalized the high draw but had said nothing concerning the low draw.

The subject's illustration shows clearly that this ambiguous wording referred to the two diverse elements mentioned above.

A somewhat similar case of negative transfer occurs in the transition from Series 3-4 to Series 3-5 in the record of Subject xiii. The reader will recall that the groups of primary critical numbers are always composed of L critical numbers each, and that the interval between such groups of critical numbers is always equal to H. When, therefore, a new series is formed by adding 1 to the high draw of the old series, the interval between successive groups of primary critical numbers is increased by 1. The groups of critical numbers, however, remain unchanged in size. But if a new series is formed by adding 1 to the low draw of the old series, the magnitude of the groups of critical numbers is increased by 1 and the interval between successive groups remains unchanged. The subject's error at this point consisted in applying the latter principle instead of the former. At the conclusion of the preceding series he had said: "The critical numbers are multiples of 7 and 1 and 2 beyond these multiples." At the presentation of Series 3-5 he said immediately: "This will be multiples of 7 and 1, 2 and 3 beyond." This attempt to apply the wrong principle is the only plausible explanation of the negative transfer which occurred at this point. There were no interruptions and no apparent fatigue. Such incorrect choice among principles which are but vaguely understood and insecurely held in mind recalls the difficulty encountered in the attempts to generalize upon the elements of a series in the early and unstable stages of their abstraction. These facts emphasize the importance of thoroughness in learning for positive transfer through generalization of experiences, and point to the danger of negative transfer from vagueness and under-learning.

4. *Summary*

The ability of subjects to solve the later problems of the experiment was much modified by the effects of learning in the mastery of earlier problems. Both facilitating and inhibiting factors were operative, though, on the whole, the former so far predominated as to leave a large balance of positive transfer.

This is true whether successive problems in a series, successive series of the same order, or successively higher orders of series are taken as a basis of comparison.

So far as our analysis goes, the greater the objective similarity between successive problems or series of problems, the higher is the degree of transfer. But it is not always possible to distinguish between objectively identical elements and common elements which owe their identity to the generalizing activities of the subject in apprehending them. It is, however, clear that a very large portion of the transfer observed, was effected through the medium of common elements of the latter sort, i.e., through concepts, and general principles, methods, attitudes, etc.

Some difficulty was encountered in the later series in attempts to apply generalizations from preceding ones. Misapplication of such generalizations often resulted in considerable loss of time and effort. False analogies, which were found to be the principal source of erroneous applications, resulted in the main from incompleteness of analysis either of the old problems upon which the generalizations were based, or of the new problems to which they were to be applied. Another source of negative transfer was found in the occasional ambiguous wording of generalizations, resulting in either their distortion or misapprehension.

There was, on the whole, much more transfer from the first to the second series in the work of subjects whose initial speed of progress was slow than in that of subjects who were more speedy at the start. This, apparently, was due in large measure to differences in the thoroughness of the acquaintance acquired with the elements of the first series.

G. Effect of Age and Education

Though the data at hand do not warrant a separate discussion of the effects of age and education on each of the processes of abstraction, generalization, and transfer, it may be worth while briefly to present such data as we have regarding the influence of these factors upon the general ability to solve our problem. Only two of our subjects differed enough from the general level of the group to warrant any attempt at comparison upon this point.

One of these subjects was in the preliminary group. The other is Subject iii of our major group of subjects.[44] The record of the latter will be mentioned first.

Subject iii of Group I was a thirteen-year-old boy who was in about the middle of his first year in high school. "I believe he is the brightest boy I have ever known," was the reply of his teacher in mathematics when asked concerning the mental ability of the boy. The father of this boy has shown remarkable genius in the business world. A comparison of the record of this subject with those of nine other subjects of Group I can be made by turning back to Table III. It will be noticed that Subject iii solved all the problems of the entire experiment in 8,425 seconds —considerably less time than was required by any other subject. Subject i, the next speediest in point of time, required 11,215 seconds, or 34.3 per cent more time than was required by the younger competitor. However, Subject i required only 165 trials whereas Subject iii required 388. Subject ii also finished with fewer trials than were required by Subject iii. Again, fewer individual problems were solved by each of these adult subjects than by Subject iii before a satisfactory generalization for all series could be formulated. Thus the lead in time which Subject iii gained over Subjects i and ii was lost in the greater number of reactions required for the attainment of a given degree of mastery of the problems. Subject iii appeared to be less conscious of method, somewhat more reliant upon trial-and-error procedure, and quicker in reaction than Subjects i and ii; but in no respect did his responses vary so much from those of these two adults as did those of some of the other adult subjects.

The other boy who participated in the experiment was eleven years old and had just completed the work of the seventh grade in the elementary schools. This subject served in the preliminary experiments in which fewer of the discontinuous series were given. His record upon these series is not, therefore, strictly comparable with those of any of the later subjects. Fortunately the father of this boy, a doctor of philosophy, solved the same problems as the son in the same order. Hereafter we shall refer to the son as K and to the father as J. The only difference in the procedure with these two subjects was that J did the work in two sittings on successive days whereas K worked five successive days but with much shorter periods. It does not seem probable, however, that the relative speed of progress of the two subjects was seriously affected by this difference in the length of periods of work, since J stopped work in each instance as soon as he began to feel fatigued. The records of these two subjects are given in Table XVI.

---

[44] With the exception of one senior college student all of the adult subjects of Group I were graduate students or instructors at the University of Chicago.

TABLE XVI

Summary of Records of Subjects K and J.

	Subject K		Subject J	
Series	No. Trials	No. Problems Solved	No. Trials	No. Problems Solved
1-2	34	9	34	17
1-3	13	6	18	6
1-4	1	1	0	0
1-5	0	0	0	0
2-3	4	2	17	10
2-4	3	2	—[45]	—
2-5	8	2	4	2
3-4	2	2	10	8
3-5	3	1	0	0
3-6	0	0	0	0
5-6	1	1	0	0
3 or 1	6	4	5	5
2 or 6	22	16	8	8
3 or 9	13	9	6	9
2 or 7	36	21	14	16
Total	146	76	116	81

[45] Accidentally omitted.

There is no marked difference in the work of these two subjects. They required the same number of trials for the first series, after which K took the lead throughout all of the remaining continuous series but was surpassed by J in the discontinuous series. However, the lead established here by J was so small that it might easily have been lost if the work of the two subjects had continued further along parallel lines.

It is worthy of note that this eleven-year-old boy solved all of the continuous series in 69 trials though the best record established by any member of group I, that of Subject ii, was 79 trials. The differences in procedure in the work of K and that of Subject ii were in the length of work periods and in the fact that K (as also all other subjects who served in the preliminary experiments) did not have the stop-watch, dictaphone, and metronome before him and he worked with matches instead of beads. The effect of these differences upon the rate of progress was probably not great.

Considering the complexity of the mental processes involved and the limited amount of data at hand, it would be rash to attempt an explanation of the essential equality in the achievements of these children with those of the most capable members of our highly selected group of educated adult subjects. Further experiments are now being undertaken upon comparable groups of children and adults with a view to the analysis and comparison of their reactions to various sorts of novel problems.

IV. DISCUSSION AND CONCLUSIONS

For convenience of treatment we have divided the learning process here under study into three stages: (1) the abstraction of elements from the problematic situation, (2) the combination of essential elements into higher conceptual units, and (3) the application of these higher unitary processes to situations other than those out of which they arose. Some elements advance into the later stages before others emerge into the first, and some telescoping of successive stages occurs with many of the elements, so that there is considerable overlapping of stages in the process of solution as a whole. Yet these stages are characteristic enough of the growing adjustment to individual elements and to the problem as a whole to warrant their separate treatment.

Each stage begins with diffusion and multiple response, and progresses through elimination and the preservation and gradual co-ordination of essential responses. Trial and error is the method of procedure in all stages, but the materials among which variations occur are somewhat different and the field of variation is progressively narrowed in the later stages.

A. *Abstraction of Elements.*—Probably none of the elements of the problems were wholly new to any of the subjects but the essential elements were sufficiently mingled and fused with unessential ones to require considerable analytic activity before the solution of a series was possible. The prevalence of random manipulation in the first stage and the notable failure of all attempts at anticipatory analysis, are in accord with Ruger's observations upon the close external similiarities existing between human and animal methods of learning.[1] Our data enable us, in a modest measure, to carry the comparison beyond the superficial resemblances into the selective factors which determine the course of learning. This is not an easy task, however, owing to the diversity of opinion among experimental psychologists concerning the selective factors in animal learning, and to the necessarily tentative nature of inferences as to the factors involved in so limited a study as our own.[2]

---

[1] *Op. cit.*, p. 12.

Perhaps the most important factor in the abstraction and selection of the essential elements of our problems, is frequency of repetition of responses to those elements. It has been shown (see above, pp. 40-47) that attention quickly came to be largely dominated by those elements of the problematic situation which were most often repeated. Likewise those elements which served most frequently as stimuli were the first to acquire definite meanings and special value for purposes of control.

Usually the most frequent objective element of response was the subject's notation of the number of beads remaining before each of his own draws. Though this type of counting did not occur with all subjects—at times some counted the numbers already drawn out by either the subject or the experimenter or by both, or seemingly did not count at all—yet it appeared to be the dominant form of response of the rapid learners, and indeed of all learners during periods of rapid progress. This counting reaction to the various numbers presented was clearly a basic element in the formation of the associations upon which the critical-number meanings were founded. It cannot be maintained that the frequent repetition of this important element of response was merely an effect rather than one of the causes of learning, since the frequency of its occurrence tended rather to be diminished than to be increased by learning.[3]

Some doubt has been raised as to the possibility of change in the order of responses in maze learning through the operation of frequency factors.[4] Even if the argument is well-founded, it is difficult to see how it could affect the explanation of learning in the present study, since both the direction of attention towards the critical numbers and the growth of the critical-number meanings may well be regarded as the effect of the mere formation and strengthening of associations between simultaneous or successive elements of stimulation and response. There is no necessity for change in the order of functioning of these associations.

---

[2] See Watson, "Behavior," pp. 267-268, and Joseph Peterson, "The Effect of Length of Blind Alleys on Maze Learning," Behav. Mon., 1917, 3, No. 4.

[3] Speaking of the status of frequency and recency as factors in maze learning Joseph Peterson says: "It cannot be too strongly pointed out . . . that this increasing percentage of reactions agreeing with the expectations based on recency and frequency effects, as learning advances from the first random stages toward the establishment of a regular habit, cannot be safely regarded as evidence that learning is brought about by recency and frequency factors: our evidence seems to justify the contrary conclusion, that this increase in reactions favoring recency and frequency factors is the *result* of the learning." (Peterson, Joseph. "Frequency and Recency Factors in Maze Learning by White Rats," p. 359.)

It need hardly be said that we do not mean to contend that analysis proceeds upon the basis of the principle of frequency alone. As previously pointed out, the order in which the various critical numbers were abstracted is almost the exact order of their nearness to the goal, or end of the trial. This is in accord with the observations upon the maze-learning of rats. Commenting upon Hubbert's experiments, upon the basis of which backward elimination had been denied, Carr says:

> Determining the average number of trials necessary to eliminate each cul de sac, the order of elimination for the entire group was 6-5-3-4-2-1, where the successive errors are numbered in order from the entrance. This order gives by the rank method a positive correlation of .943 between quickness of elimination and propinquity to the food box. The average number of trials necessary to eliminate the last three errors was less than that for the first three for 90 per cent of the rats. Surely there is a very pronounced tendency for the errors to be mastered in proportion to their nearness to the food box, and the deviation from the *exact* correlation for each rat may well be due to the operation of other causal agencies.[5]

In summarizing her data concerning the order of elimination of errors, Vincent says:

> The elimination of the final members of the series first is not only true of the groups as a whole but also of the individual animals.[6]

---

[4] *Ibid.*, pp. 347-348.

[5] Carr, H. A. "The Distribution and Elimination of Errors in the Maze," Jour. Animal Behav., 1917, vol. 7, p. 146.

[6] Vincent, Stella B. "The White Rat and the Maze Problem," *Ibid.*, 1915, vol. 5, p. 371. Regarding the applicability of Vincent's data Hubbert and Lashley say: "In the form in which this is presented, however, Vincent is

Peterson finds the same general backward elimination of errors,[7] and even Hubbert and Lashley agree that

> when the averages of very large groups of animals are taken, there does seem to be a progressive elimination of errors from the food compartment to the entrance of the maze.[8]

What are the factors upon which the regressive order of mastery of the elements of the maze and of our own problems, is based? Though some of the suggested agencies in the determination of the order of learning in the maze are obviously inapplicable to our problems, it is not improbable that the determining factors are in part identical in the two forms of learning. It may be said at once that we do not assume with Watson, that the demonstration of a backward order of mastery need give any comfort to the advocates of the retroactive effects of *pleasure* as a causal factor in learning. As a matter of fact, the goal of most trials of our subjects was defeat; and here, as elsewhere, defeat usually gave rise to considerable evidence of disappointment or annoyance.

It may be argued that no explanation is necessary, beyond the greater frequency of response to lower than to higher critical numbers. This argument would be in essential agreement with Peterson's explanation of the order of mastery of the elements of the maze.[9] That other factors entered into the determination of the order, however, is clearly evident from the records of some of the subjects of Group III. While wrestling with 14 beads as their initial problem in Series 1-2, some of these subjects made nearly all of their errors in drawing from 14, or 11 beads; and, in consequence, reacted practically as often to 9 as to 6 beads. Yet 6 was recognized as a critical number some time before 9 was so regarded. Further evidence that the order of

---

not justified in applying the data to the problem. She makes no statement as to what constituted a trial in her experiments and in two 'typical records' . . . we find that the rat, after reaching the food, was allowed to return and re-explore the maze." (*Ibid.*, 1917, vol. 7, p. 132.)

[7] *Op. cit.*, pp. 360-361.

[8] Hubbert and Lashley, "The Elimination of Errors in the Maze," Jour. Animal Behav., 1917, vol. 7, No. 2.

[9] Peterson, Joseph, *op. cit.*, pp. 338-346.

learning was affected by the relative nearness of the elements to the goal, is found in the records of two subjects for whom the procedure was so modified as to eliminate the frequency factor, except in so far as this factor is itself determined by proximity to the goal.[10] All of the critical numbers learned by these two subjects were learned in the exact order of their nearness to the goal.

Attempts to solve the higher numbers by merely hitting upon a correct sequence of draws by a sort of long-range trial and error, had played a minor rôle in learning in the former experiments, but such attempts were practically useless here. Progress in analysis, which had previously been confined to the elements near the goal, was here almost entirely so confined.

At the beginning of their work our subjects (and this is true of all the groups) were able to foresee the outcome of a trial from a distance of not more than the first or, rarely, the second critical number above the goal, and that only with some doubt and uncertainty. But with continued repetitions the foresight of consequences became clearer and more certain until finally the critical number next above the goal became so closely associated with defeat as to constitute a new point of orientation from which to push the analysis back one step farther. Thus, after its discovery and a sufficient number of repetitions to associate it firmly with the next lower critical number, each critical number became in effect a new goal. Two important consequences follow from this fact: (a) the next higher critical number was brought within range of direct and more or less clear apprehension of the consequences of each of the possible reactions to it, and (b) the time intervening between the occurrence of each of the higher critical numbers and the full realization of defeat, was reduced by a number of seconds.

---

[10] The first of these subjects, a boy of fourteen years of age, worked upon 30 beads in Series 1-3 as his initial problem. To avoid the suggestive sequence of critical numbers in exact multiples of 4, success in a trial was defined as *forcing one's opponent to draw last*. The critical numbers then become 1, 5, 9, 13, etc. Again, the simple, suggestive sequences of the experimenter's draws, followed in the earlier experiments, were abandoned in favor of more difficult sequences which would tend to resist formulation and so impel the subject to attend more exclusively to the number remaining before each draw.

This problem was found to be too difficult. After 73 trials requiring upwards of two hours of time the subject had discovered only *5*, *9*, and *13* of the critical numbers above 1, and had made practically no progress in the organization of the few facts observed.

A simpler problem, consisting of a similar variation of Series 1-2 with 25 as the initial number, was therefore given to the second subject. This subject succeeded, after 72 trials occupying a little less than two hours, in discovering all the critical numbers in his problem and in formulating his discoveries into a suitable generalization for the entire series. The number of draws from the various critical numbers was here almost exactly uniform, and all of the critical numbers were discovered in the exact order of their magnitude.

It may be argued that (a) would result merely in the arousal of a new type of reaction to the critical number next above the new point of orientation, which, by frequent repetition, would gradually get itself established as the invariable response. This fact is freely admitted. But since the arousal of this sort of response is directly dependent upon the proximity of its stimulus to the goal, its repetition can hardly be used as evidence that frequency factors are alone responsible for the regressive order of mastery of the elements of the problem. On the contrary, the retroactive influence of the goal in selecting and consolidating the closely preceding essential reactions is here clearly evident.

Whatever causal agencies may have been involved, there can be no doubt that the rate of formation of associations between the goal response (the positive recognition of defeat in the current trial) and the various critical numbers varied directly with the relative nearness of these numbers to the goal. There seems to be no good reason why association should not be facilitated by a close temporal approximation of the various elements to be associated. That such facilitation does occur is suggested by certain experimental observations by Carr and Froeberg upon animal and human learning respectively. Carr says:

> The final co-ordination consists of an association between each act and the sensory aspects of the preceding act as well as a distinctive motor attitude resulting from the same. The relative efficiency of the two stimuli in determining the choice varies with the individual. The problem was mastered quickest by

those animals that relied mainly upon the factor of motor attitudes in making their choice. This fact suggests the hypothesis that the speed of learning is to some extent a function of the degree of temporal contiguity between the terms to be associated.[11]

Froeberg studied the effects of varying the length of the interval between the presentation of nonsense syllables in pairs, the interval being occupied by the repetition of two-digit numbers. Commenting on his results he says:

> There is thus a distinct, though irregular, decrease in the number of right responses as the interval between the numbers of the pair in successive presentation increases.[12]

This view of the effect of the degree of temporal contiguity upon the speed of association is in accord with the close relation already observed between the speed of reaction of our subjects and the quickness of their discovery of the principle of drawing opposites. Moreover, the operation of such contiguity factors is evident in the fact that subjects commonly learned to recognize the critical status of numbers some time before they were able to give a reason for it.

Thus in dealing with novel situations through the abstraction of essential situation- and response-elements and the organization of these elements into serviceable concepts, mere frequency of repetition appears to play a rôle of no less importance than in the selection and combination of essential elements of stimulus and response into effective sensori-motor co-ordinations. The mechanical operation of frequency and of the factors mentioned in (a) and (b) above appears also to be quite as effective in determining the order of mastery of the elements in this type of conceptual learning as it has been supposed to be in sensori-motor learning.[13]

---

[11] Carr, H. A. "The Alternation Problem," Jour. Animal Behav., 1917, vol. 7, No. 5, p. 384.

[12] Froeberg, Sven. "Simultaneous versus Successive Association," Psych. Rev., 1918, vol. 25, No. 2, p. 162.

[13] Under the conditions of maze learning the process mentioned in (a) need involve only direct responses of the conditioned-reflex type to present stimuli. This statement would appear to imply the operation of "retroactive association," the possibility of which has been questioned by Hubbert and Lashley (*op. cit.*) and by Peterson (Joseph Peterson, "Frequency and Re-

B. *Combination of Essential Elements.*—In the early stages of the analysis of our problems, as at the beginning of the learning of telegraphy, typewriting, chess playing, etc., the individual elements are apprehended in relative isolation. Attention to one element precludes attention to others at the moment, with the result that the responses are of a random, haphazard character. With the progress of learning the span of attention is gradually broadened by the combination and organization of individual elements into larger, more meaningful units through which it is possible to focus one's relevant experiences upon new situations with a minimum loss of time and effort.

Regarding this process of combination and organization of essential elements Cleveland says:

> How organization can best be brought about is still an open question. . . . Its ultimate nature we do not know. To a great extent the material organizes itself, i.e., the organization is physiological and a matter of growth.[14]

That this organization is very largely independent of explicit conscious direction is indicated by the fact that the critical status of numbers was usually recognized before the subject was able to give a reason for it; that the prinicple of drawing opposites

---

cency Factors in Maze Learning by White Rats," Jour. Animal Behav., 1917, vol. 7, No. 5) though it is not entirely clear what connotation is to be given the term. If a "pleasurable situation" at the end of the series of responses to be learned is an essential factor, it is quite apparent that there was no retroactive association operative in the learning of our subjects. But the inclusion of this factor was seemingly not intended—though one cannot but suspect that the aversion to the idea of retroactive association arises from its early complication with the controverted stamping-in effects of pleasure—since, after its definition in terms of reactions leading up to a pleasurable situation, the process is restated in terms of conditioned reflexes.

Hubbert and Lashley's evidence against the presence of retroactive association in maze learning is, to say the least, inconclusive. The effect of the marked progressive shortening of the true runways toward the center of the maze was not determined. Peterson (Joseph, *op. cit.*, p. 361) has pointed out two other sources of weakness in their argument. Yet he writes (p. 362): "There seems to be no 'retroactive association' necessary, as Hubbert and Lashley rightly conclude." His evidence for this more conservative view is, however, drawn from his own experiments.

[14] Cleveland, A. A. "The Psychology of Chess and of Learning to Play It," American Journal of Psychology, 1907, vol. XVIII, p. 297.

was often followed and sometimes clearly formulated before the reason for it was known, and that solutions for series sometimes obtruded themselves upon a subject's consciousness after a period of rest during which no thought had been given to the problem and prior to which the subject's grasp of the elements of the problem had been characterized by confusion and lack of organization. Moreover, the unreasoned recognition of the principle of drawing opposites, and the backward order of learning of critical numbers, are clearly explicable on the basis of frequency of repetition and close temporal contiguity of the elements which were associated, as already shown.

The essential elements were unified not so much by direct association from one element to another as by the association of each element with a common intervening symbol. The character and manner of selection of such a symbol is a matter of some importance. Sometimes, during the early stages of unification, the intervening symbol is merely one of the elements of the concrete situation, or a direct response to such a situation-element, which happens to occur more frequently than others or to possess some other advantage, perhaps of intensity, duration, or position. Thus the goal of the trial, or the attitude aroused by defeat, or the verbal expression of the latter appeared to serve as a common symbol in the early stages of analysis and organization.

But as soon as enough numbers had been classified as "losing," "impossible," or "insoluble" to give the subject a general schematic impression of the character of the problem, the efforts at organization began to proceed more largely through the arousal and application of old concepts which functioned as a crude sort of hypotheses. Here the associations between the elements and the symbol were already in existence, provided the conception, or hypothesis, was correct. Henceforth the problem of unification became more a matter of calling up and testing out of various trial hypotheses than of the formation of new associations.

The various concepts which, under the influence of the general conception of the problem, functioned as hypotheses, appear to have been aroused in three fairly distinct ways: (a) through their association with a specific element of the problem, (b) through the combined associative pull of a number of disconnected elements observed in rapid succession, and (c) through their association with *relations* observed to exist between individual elements, or, perhaps, with symbols representing such relations.

As an Example of (a), a subject has conceived of 9 as an insoluble number. It occurs to him that 9 is an odd number or that it is a multiple of 3, and he straightway infers that all odd numbers or all multiples of 3, as the case may be, are insoluble. This is the all but exclusive manner of origination of hypotheses in the early stages of learning when, owing to a lack of acquaintance with the elements of the problem, the span of attention to the elements is very narrowly limited. Subjects who have passed beyond the early stages of learning also revert to this form under the stress of emotion, self-consciousness, or fatigue, which tend seriously to narrow the span of attention to the elements of the problem. There are some individuals who, through native limitations of the span of attention seem doomed to a marked dependence upon this primitive form of arousal of hypotheses. These subjects were prone to react hastily. They drew rapidly and, disregarding recently observed and often well-known facts, frequently jumped to unwarranted conclusions from which they showed little ability to extricate themselves by crucial tests. That the limitation of the span of attention exhibiting itself in this lack of inhibition is probably native is indicated by the fact that the reactions of these subjects continued to be relatively rapid and ineffective even after an acquaintance had been gained with a fairly large number of elements. There are, of course, some subjects who are able to deal both rapidly and effectively with a wide variety of elements. Subjects iii and xi were the most notable examples of this type.

The origination of hypotheses in the manner described in (b) was observed in the later stages of learning, particularly in the work of subjects who inhibited the primitive tendency to generalize upon every instance and were intent upon the discovery and retention of several critical numbers prior to generalization.

With repeated recall of such previously observed critical numbers there was an increased facility manifesting itself in greater speed of repetition. Finally, after one or two repetitions of several critical numbers in rapid succession, the subject would suddenly announce his belief that all multiples of 3 are insoluble. Likewise, the later discovery of the superficial relation of critical numbers to the sum of the L- and H-draws often appeared to arise from the combined associative pull of a number of rapidly reviewed instances the essential feature of which as separate instances were not clearly apprehended. Here also, as in dealing with the critical numbers of a single series, the first general formulation was usually given in a tentative manner and further evidence sought for its proof. But the further evidence by which proof was established was usually of exactly the same type as that which had preceded the formulation, namely, the enumeration of more confirmatory instances and the absence of refractory ones.[15] Sometimes hypotheses originated in explicit analysis and comparison of the elements of one or more series. This is the type of origin mentioned in (c) above. At its best this process involves a clear insight into the causal relations of the phenomena in question and carries its own confirmation. At its worst it results in false analogies of the sort mentioned in the discussion of negative transfer (see above, p. 95 ff.). This process is dependent upon a fair acquaintance with the elements of the problem and is therefore impossible in the early stages of adjustment to really novel situations.

But the function of incipient hypotheses was not confined to the mere unification of previously discovered elements. These trial conceptions served also to direct attention to hitherto unknown elements and so aided materially in the discovery of new data.[16] The latter function is particularly noticeable in case of such concepts as even and odd numbers, every other number, multiples of 3, etc. The verification of such an hypothesis through its appliation to previously known data and its functioning in the discovery of new facts are but slightly different phases of the same process of application. Both can be explained upon the basis of the laws of habit.[17] For example, suppose that 15 is under consideration and is conceived as a multiple of 3. Other multiples of 3 are immediately called to mind by association. But there is no invariable sequence. Sometimes, in accordance with the habit of counting upwards, 18 and 21 are the first associates aroused; sometimes the first associates to be aroused are 12 and 9, owing perhaps to the recency of their repetition. Simple associations of this sort probably account for much of the directive value of the more elaborate hypotheses of science.

---

[15] *Cf.* Dewey, "Studies in Logical Theory," p. 174.

[16] *Cf.* Dewey, *op. cit.*, p. 145.

There is a notable difference in the explicitness and stability of hypotheses in different stages of their development. Incipient hypotheses often occur in the early stages of the work as fleeting and perhaps vague insights. Further familiarization with the elements of the problem gives greater explicitness and stability to these hypotheses, though lapses still occur as is evidenced by the subjects' failure to make even the simplest and most obvious applications. A degree of instability sometimes persists long into the later stages of the experiment owing to the rivalry of conflicting or superfluous hypotheses which, though entertained earlier in the game, may have been neglected in the meantime. Book found a similar instability in the building up of co-ordinations in typewriting. He says:

> It was observed by the learners that the older and more elemental habits used in the earlier stages of writing tended strongly to persist and force themselves upon the learner long after they had been superceded by higher-order habits. At every lapse in attention or relaxation of effort, the older habits stepped forward, as it were, and assumed control, thereby tending to perpetuate themselves. Only when a high degree of effort was being permanently applied . . . was attention forced to lay hold of the higher and more economical methods of work." Referring later to these fluctuations of attention and performance he says: "They were wholly beyond the learner's control. He

---

[17] This is in accord with Thorndike's view that "learning by inference is not opposed to, or independent of, the laws of habit, but is really their necessary result under the conditions imposed by man's nature and training." (Thorndike, E. L. "Educational Psychology," vol. II, p. 36.)

could not avoid them and could do little to regulate or control them."[18]

Continued attempts to deal with similar materials brought about a gradual strengthening of the associations necessary for the more ready arousal of appropriate concepts and the consequent failure of inappropriate ones to suggest themselves. This gradual automatization of the higher conceptual responses was observed by Cleveland in his study of chess. After giving an account of the combination of significant elements into larger complexes in which general terms "built up step by step" became increasingly more prominent, he says:

> We are in the habit of speaking of the automatic in the motor realm, meaning by it that certain movements or combinations of movements are carried on without conscious guidance. Is there such a thing as automatism in the realm of the purely intellectual? It seems to me that this question is to be answered in the affirmative. There is something in the purely intellectual life corresponding to motor automatism, which is shown in the ability to think symbolically or abstractly, and thus to handle large masses of detail with a minimum of conscious effort. It involves the increasing ability to take in during a single pulse of attention a larger and larger group of details which means, of course, that the attention is no longer needed for each one.[19]

The combination of elements entering into a concept through various stages of automatization, is well illustrated in Fisher's study already referred to. Her subjects were presented with a series of ten somewhat similar figures exposed in succession for three seconds each. Each group was given a nonsense name, and the task consisted in defining the group name after the observation of the series. When repeated exposures revealed no additional essential features the exposures were discontinued; but the subjects continued at later sittings to recall the essential features of each group and report them as usual. Regarding the automatization of these concepts the author says:

> Functionally viewed (i.e., regarded from the point of view of the difficulty and effortfulness, or the ease and mechanizedness

---

[18] Book, "The Psychology of Skill," pp. 94, 122.

[19] Cleveland, "The Psychology of Chess and Learning to Play It," American Journal of Psychology, vol. 18, p. 300.

with which the concept-meanings entered consciousness), the recalls ranged from an initial form in which more or less hesitation was present, to a final form which was marked by a high degree of mechanization and where the spoken statements followed in uneventful fashion, either immediately upon the instructions themselves, or upon a brief and transitory visual or verbal image which served to 'set off' the train." Describing the final stage of the developments of the concepts under observation she continues: "At this stage the experience of generality was nothing more than an unhesitating, ready, and even mechanical mentioning of the general features. . . . The generality experience was based essentially upon nothing more than a highly mechanized association between the words 'Zalofs are objects having,'—or 'Zalofs always have,'—and the enumeration of the essentials. The recalls were often given on a very automatic fashion."

In recapitulation it may be said that, in the early stages of learning, those elements which occurred most frequently and in closest temporal contiguity were generally the first to be combined into higher units. Certain situation- and response-elements which occurred most often and in closest proximity to other elements, became so closely associated with the latter as to serve as symbols through which the various elements were coordinated and their subsequent recall much facilitated. In the later stages of learning symbols representing well-organized concepts were called in by association. Through the medium of these symbols, and again by means of specific associations, meanings were transferred from previously known to newly discovered elements. Associations of this sort at first functioned slowly and imperfectly, but continued repetition brought about a facility and even automaticity of functioning comparable to that of sensori-motor co-ordinations.

C. *Application of Knowledge.*—As previously mentioned no marked tendency was observed for perceptual solutions to find application beyond the limits of the specific concrete situations in which they occurred. This absence of transfer was attributed to the fact that responses were made *directly* to the objective situation rather than indirectly through the intervention of a symbol. The solution of numbers beyond the range of direct perceptual control showed, on the other hand, a very marked tendency to apply to new situations. This fact was attributed to the intervention of symbols, which was necessitated by the extent and complexity of the materials to be controlled in the attainment of a solution.

The reason for the superior applicability of solutions involving the use of symbols is not far to seek. The numerical symbols used in counting the beads remaining before each draw have been more frequently repeated than the concrete situations underlying the perceptual solutions and are therefore more easily recalled. Because of the greater ease of recall of often-repeated symbols than of seldom-observed situations, whatever meanings have been detached from the latter and associated with such symbols are more easily aroused in the presence of new situations. This advantage in the facility of recall of general symbols is important in as much as the conditions of application generally require the recall of the materials to be applied, while attention is directed primarily to the present problematic situation.

But it is not through the greater ease of recall of meanings which have grown out of the problem situation and become attached to the symbols so much as through the arousal of previously formed associations that such symbols serve to facilitate the application of old experiences to new situations. The symbols here employed in the reaction to all problems above 6 or 7 beads, were necessarily numerical symbols which were already organized into various orderly sequences. The moment such a symbol is used each of these sequences tends to be aroused through association, and thus to extend the meaning of the term under momentary consideration to the other terms of the suggested series. Thus 12, pronounced insoluble and conceived as an even number, tended strongly to suggest the insolubility of other even numbers such as 10 and 14. But, conceived as a multiple of 3, it tended to suggest the insolubility of 9, 15, etc. The tendency for meanings to apply themselves to new data through the medium of such general symbols was often so strong as to distort the memory of frequently observed facts.* Thus one of the preliminary subjects, observing the insolubility of 18 beads in Series 1-2 and conceiving it as an even number, immediately lost his somewhat feeble grasp upon previously observed facts and "remembered" positively that 16, 14, 12, and 10 were also insoluble numbers.

The range of application is obviously determined largely by the generality of the symbol through which transfer[20] is to be effected, i.e., by the number and variety of *particulars* represented by the symbol. When, for example, the critical-number meanings were associated with 6, they were applicable to but one number in a single series. When these meanings became associated with the more general symbol, *a-multiple-of-3,* they became applicable to all numbers of the first series. Finally, when the still more general symbol, $L + H$, was substituted for the 3 of the earlier formula, the associated meanings became applicable to all problems of all continuous series. This is in accord with Judd's view that application is much facilitated by generalization.[21]

It is of course possible for knowledge to be expressed in general form without necessarily being generalized. There is, as Dewey observes, "always danger that symbols will not be truly representative; danger that instead of calling up the absent and remote in the way to make it enter a present experience, the linguistic media of representation will become an end in them-

---

[*] This tendency was probably strengthened in our experiments by the general conception of the problem.

Speaking of the influence of specific associations upon thinking, Thorndike says: "It has long been apparent that man's *erroneous* inferences—his unsuccessful responses to novel situations—are due to the action of misleading connections and analogies to which he is led by the laws of habit. It is also the fact, though it is not so apparent, that his successful responses are due to fruitful connections and analogies to which he is led by the same laws." (Educational Psychology, vol. II, p. 48.) The operation of the law of habit is here, however, as obvious in the arousal of correct as of incorrect responses through the medium of the general symbol.

[20] Bode argues that "transfer of training, when translated into terms of presentday knowledge, means the extension or applicaiton of meanings to new problems or new situations." (Boyd Henry Bode, "A Reinterpretation of Transfer of Training," Educational Administration and Supervision, vol. V, 1919, p. 107.)

[21] Judd, C. H. "Psychology of High-School Subjects," pp. 392-435.

selves."[22] Even though the symbol may previously have acquired a wide range of meaningful associations, it may, through the subject's lack of acquaintance with the elements of the present situation, function as a mere verbal response. Thus the hypothesis that that all multiples of 3 are insoluble often failed to function in any noticeable degree until long after its first explicit formulation, if such formulation chanced to occur before a fair acquaintance with the elements of the situation had been gained. That the degree of acquaintance with the elements strongly affects the applicability of generalizations to new problems, is also suggested by the fact that the degree of transfer from Series 1-2 to Series 1-3 was much greater for subjects whose progress through the first series was slow than for those who progressed rapidly.

The discovery of new elements of the situation through the associative action of a general symbol and the transfer of old meanings to these elements through the same medium, must inevitably lead to errors, since there is no certainty that the required symbol will be the first aroused or that when once aroused this symbol will call up only the correct association. Trial and error is obviously an essential method of procedure so long as any of the elements of the old or the new situation remain unknown, i.e., whenever new applications are being attempted. If the old situation is not fully analyzed, the generalization may be erroneous or inadequate; if analysis of the new situation is not complete, nonessential elements may dominate attention and thus bring about the arousal of inappropriate conceptions.

From the foregoing facts it is apparent that transfer is much facilitated by explicit generalization of experiences. This facilitation can be accounted for on the basis of the operation of specific associations between the symbols required for generalization and the elements of direct experience, which furnish the materials for generalization. The degree of transfer appears to depend upon both the number and the strength of such associations. It is also dependent largely upon the degree of acquaintance with the elements of the problematic situation to which application is to be affected. The educational value of a habit of analyzing and generalizing experiences, which may, according to Judd,[23] be developed by constant exercise of those functions, is beyond question. But it should not be forgotten that this habit must function in more or less familiar materials and through the operation of specific associations. Its value depends far more upon a gradual ordering of daily experiences, through repeated attempts at analysis and generalization, into such a flexible, systematic form as will make them available for future use, than upon sudden transformation of experiences at the moment of need.

---

[22] Dewey, J. "Democracy and Education," p. 272.

D. *General Conclusions.*—Trial and error appears to be a universal method of procedure in learning of the problem-solving type. Not only does this procedure dominate the early stages of analysis of new materials, but it is a conspicuous factor in the determination of progress in the generalization of knowledge and its application to new situations. The field of variation is gradually limited through the effects of generalization, and the testing out of trial responses is facilitated through the familiarization of the elements of the situation. Thus larger and larger units of response come to be represented ideationally and tested out either overtly or in imagination; but the general trial-and-error character of the process remains always the same.

The most obvious factors in the selection and accentuation of essential elements were frequency of repetition of elements and their relative nearness to a goal, or end of action. Generalization and application of experiences were apparently somewhat less, though still largely, controlled by the same factors.

Progress in the detachment of elements and their generalization and application to new situations can be traced largely to the gradual formation and automatization of specific associations and to the associative arousal of previously formed concepts. But it should not be forgotten that old concepts wrung into service in this manner are themselves the product of earlier processes of gradual formation and mechanization of associations, essentially similar to the learning of sensori-motor co-ordinations. The learning process is not so much modified as abridged by this action of old concepts. The difference between sensorimotor learning and learning through abstraction and generalization is not so much a difference of method as of the type and complexity of previously established inter-relations of the materials to be organized. All of our data appear to confirm the view recently expressed by Thorndike that "Thinking and reasoning do not seem in any useful sense opposites of automatism, custom, or habit, but simply the action of habits in cases where the elements of the situation compete and co-operate notably."[24]

---

[23] *Op. cit.*, pp. 432-435.

All ideas of theoretical importance which are expressed in the foregoing pages have been stated previously by other writers. The only originality which is claimed for the present study is to be found in the experimental verification of some of these ideas and in the development of a technique which, it is hoped, may prove to be of value in the further investigation of some of the problems in this field.

---

[24] Thorndike, E. L. "The Psychology of Thinking in the Case of Reading," Psychological Review, vol. XXIV, 1917, pp. 233-234.

Vol. XXVIII No. 7 *PSYCHOLOGICAL REVIEW PUBLICATIONS* Whole No. 129 1920

Psychological Monographs

EDITED BY
JAMES ROWLAND ANGELL, 522 FIFTH AVE., NEW YORK
HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
JOHN B. WATSON, NEW YORK (*J. of Exp. Psychol.*)
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*) and
MADISON BENTLEY, UNIVERSITY OF ILLINOIS (*Index*)

The Higher Mental Processes in Learning

BY
JOHN C. PETERSON, PH.D.
Assistant Professor of Psychology, State Agricultural College,
Manhattan, Kansas

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.)
PARIS (16 Rue de Condé)

PRINCETON
UNIVERSITY
PRESS

ACKNOWLEDGMENTS

I am much indebted to Professors James R. Angell and Harvey A. Carr for numerous helpful suggestions and for generous and stimulating interest at all stages of the work. My thanks are also due to those who kindly gave their time to serve as subjects.

CONTENTS

I. Introduction 1

II. Technique and Procedure 3
  A. Description and Analysis of Problems Presented to Subjects for Solution 3
  B. Apparatus and Recording of Data 10
  C. Order of Presentation of Problems 11
  D. Degree of Learning and Uniformity of Procedure 13
  E. Instructions to Subjects 16
  F. The Subjects 17

III. Results 19
  A. Unit of Measurement 19
  B. Changes in the Rate and Character of Progress .. 22
  C. Perceptual Solutions 35
  D. Analysis 37
    1. Types of Elements Abstracted: Direction of Analysis 38
      a. Frequency of Repetition as a Factor 41
      b. Effect of Nearness to a Goal 47
      c. Effect of Speed of Reaction 49
    2. Explicitness and Extent of Analysis 53
    3. Time Relations of Manipulation and Analysis 56
    4. Summary 62
  E. Generalization 63
    1. Relative Absence of Generalization in Perceptual Stage 63
    2. Development of the Concept of the Critical Number 64
    3. Random Hypotheses 73
    4. Summary 81

F. Transfer 82
  1. Degree of Transfer 83
  2. Conditions of Transfer 89
    a. Objectively Identical Elements 89
    b. Subjectively Identical Elements 90
    c. Generalized Methods of Procedure 91
    d. Effect of Thoroughness of Learning upon Transfer 93
  3. Negative Transfer 95
  Summary 98
G. Effect of Age and Education 99

IV. Discussion and Conclusions 103

A. Abstraction of Elements 103
B. Combination of Elements 110
C. Application of Knowledge 116
D. General Conclusions 120

PSYCHOLOGICAL MONOGRAPHS

VOL. XXIV

102. **Two Studies in Mental Tests. I. Variable Factors in the Binet Tests. II. The Diagnostic Value of Some Mental Tests.** Carl C. Brigham. Pp. 254. $2.50. 103. **Radiometric Apparatus for Use in Psychological and Physiological Optics.** C. E. Ferree and Gertrude Rand. Pp. xvi+65. 75 cents. 104. **Transfer of Training and Retroaction.** Louie Winfield Webb. Pp. 90. $1.00. 105. **The Reliability of Mental Tests in the Division of an Academic Group.** Beardsley Ruml. Pp. 63. 75 cents. 106. **The Analysis of Mental Functions.** Curt Rosenow. Pp. 43. 50 cents.

VOL. XXV

107. **Retroactive Inhibition as Affected by Conditions of Learning.** Edward Chace Tolman. Pp. 50. 75 cents. 108. **University of Iowa Studies in Psychology.** Edited by Carl E. Seashore. Pp. 163. $1.75. 109. **A Higher Scale of Mental Measurement and its Application to Cases of Insanity.** A. J. Rosanoff, Helen E. Martin and Isabel R. Rosanoff. Pp. 113. $1.50. 110. **An Experimental Study of Attention from the Standpoint of Mental Efficiency.** Shoan Masuzo Fukuya. Pp. 48. 50 cents. 111. **The Interference of Will-Impulses.** Abraham A. Roback. Pp. 158. $1.75.

VOL. XXVI

112. **Psychology of Clothing.** George V. N. Dearborn. Pp. vi+72. $1.15. 113. **Some Imaginal Factors Influencing Verbal Expression.** Esther E. Shaw. Pp. 137. $2.00. 114. **The Learning Curve Equation.** L. L. Thurstone. Pp. 51. 75 cents. 115. **The Effect of Alcohol on the Intelligent Behavior of the White Rat and Its Progeny.** Ada Hart Arlitt. Pp. 50. 75 cents. 116. **The Form of the Learning Curves for Memory.** Conrad L. Kjerstad. Pp. 89. $1.35. 117. **An Introspective Analysis of the Process of Comparing.** Samuel W. Fernberger. Pp. 161. $2.40.

VOL. XXVII

118-122. **Psychological Studies from the Catholic University of America.** Edited by Edward A. Pace. Pp. 601. $5.50. 118. **A Study and Analysis of the Conditioned Reflex.** Ignatius A. Hamel. Pp. 66. 75 cents. 119 **Image and Meaning in Memory and Perception.** Thomas Verner Moore. Pp. 230. $2.70. 120. **The Correlation Between Memory and Perception in the Presence of Diffuse Cortical Degeneration.** Thomas Verner Moore. Pp. 49. 55c. 121. **Clinical and Psychoanalytic Studies: i. Conscious and Unconscious Factors in Symbolism.** Paul Hanly Furfey. **ii. Hypnotic Analogies.** Thomas Verner Moore. **iii. Concomitants of Amentia.** Miriam E. Loughran. Pp. 143. $1.65. 122. **The Development of Meaning.** Agnes R. McDonough. Pp. 113. $1.35.

VOL. XXVIII

123. **Quantitative Aspects of the Evolution of Concepts.** Clark L. Hull. Pp. 85. $1.35 124. **An Experimental Analysis of a Case of Trial and Error Learning in the Human Subject.** George S. Snoddy. Pp. 78. $1.25 125. **Work With Knowledge of Results Versus Work Without Knowledge of Results.** George F. Arps. Pp. 41. 65 cents. 126. **Individual Differences in Finger Reactions.** Esther L. Gatewood. Pp. 43. 65 cents. 127. **The Lag of Visual Sensation in its Relation to Wave Lengths and Intensity of Light** Marion A. Bills. Pp. 101. $1.50. 128. **Studies from the Psychological Laboratory of the University of Chicago—Some Factors Determining the Degree of Retroactive Inhibition.** Edward Stevens Robinson. Pp. 57. 90 cents. 129. **The Higher Mental Processes in Learnng.** John C. Peterson. Pp. 121.

VOL. XXIX

130. **A Qualitative Analysis of the Process of Forgetting.** Harold R. Crosland. Pp. 159

Directory of American Psychological Periodicals

American Journal of Psychology—Ithaca, N. Y.: Morrill Hall. Subscription $6.50. 600 pages annually. Edited by E. B. Titchener. Quarterly. General and experimental psychology. Founded 1887.

Pedagogical Seminary—Worcester, Mass.: Florence Chandler. Subscription $5. 575 pages annually. Edited by G. Stanley Hall. Quarterly. Pedagogy and educational psychology. Founded 1891.

Psychological Review—Princeton, N. J.: Psychological Review Company. Subscription, $4.25. 480 pages annually. Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Bulletin—Princeton, N. J.: Psychological Review Company. Subscription $5. 720 pages annually. Psychological literature. Monthly. Founded 1904. Edited by Shepherd I. Franz.

Psychological Monographs—Princeton, N. J.: Psychological Review Company. Subscription $5.50 per vol. 500 pp. Founded 1895. Ed. by James R. Angell. Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.: Psychological Review Company. Subscription $1.50. 200 pp. Founded 1895. Edited by Madison Bentley. An annual bibliography of psychological literature.

Journal of Philosophy—New York: Sub-Station 84. Subscription $4. 728 pages per volume. Founded 1904. Bi-weekly. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology—Sub-station 84, N. Y.: Archives of Psychology. Subscription $5. 600 pp. ann. Founded 1906. Ed. by R. S. Woodworth. Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology—Boston: Richard G. Badger. Subscription $5. 480 pages annually. Edited by Morton Prince. Bi-monthly. Founded 1906. Entire field of abnormal psychology.

Psychological Clinic—Philadelphia: Psychological Clinic Press. Subscription $2.50. 288 pages ann. Edited by Lightner Witmer. Founded 1907. Without fixed dates (9 numbers). Orthogenics, psychology, hygiene.

Training School Bulletin—Vineland, N. J.: The Training School. Subscription $1. 160 pp. ann. Ed. by E. R. Johnstone. Founded 1904. Monthly (10 numbers). Psychology and training of defectives.

Journal of Educational Psychology—Baltimore: Warwick & York. Subscription $4. 540 pages annually. Founded 1910. Monthly (9 numbers). Managing Editor, J. Carleton Bell. (Educational Psychology Monographs. Edited by Guy M. Whipple. Published separately at varying prices. Same publishers.)

The Behavior Monographs—Cambridge, Mass.: Emerson Hall. Subscription $5. 450 pages per volume. Edited by John B. Watson. Published without fixed dates, each number a single research.

Psychoanalytic Review—Washington, D. C.: 3617 10th St., N. W. Subscription $6. 500 pages annually. Psychoanalysis. Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology—Princeton, N. J.: Psychological Review Company. 480 pages annually. Experimental. Founded 1916. Subscription, $4.25. Bi-monthly. Edited by John B. Watson.

Journal of Applied Psychology—Worcester, Mass.: Florence Chandler. Subscription $4. 400 pages annually. Founded 1917. Quarterly. Edited by Stanley Hall and L. R. Geissler.

Journal of Comparative Psychology—Batimore: Williams & Wilkins Co. Subscription $5. 500 pages (approximately) per volume. Bi-monthly. Edited by Knight Dunlap and Robert M. Yerkes. Founded 1921.